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PHILOSOPHICAL JOURNAL,

EXHIBITING A VIEW OF THE

PROGRESSIVE DISCOVERIES AND IMPROVEMENTS

IN THE

SCIENCES AND THE ARTS.

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CONTENTS.

PAGE

1. Observations on the Genetic Cycle in Organic Nature, and particularly on the Relation between the different Forms of Alternation of Generations and the more Ordinary Modifications of the Reproductive Process. By George Ogilvie, M.D., Lecturer on the Institutes of Medicine in Marischal College and University, Aberdeen, ... 1

2. Peruvian Gleanings. By Dr Archibald Smith, . 25

3. On the Vestiges of Extinct Glaciers in the Lake Districts of Cumberland and Westmoreland. Part I. By Edward Hull, A.B., F.G.S. With two Plates, 31

4. On the Application of certain Laws of Heat and Combustion to the Use and Economy of Fuel. Being the substance of a Paper read before the Aberdeen Philosophical Society, 11th February 1859. By Alexander D. Milne, ... 45

PAGE

5. On the Disguises of Nature; being an Inquiry into the Laws which regulate External Form and Colour in Plants and Animals. By ANDREW MURRAY, F.R.S.E., ... 66

6. Notes on the Aurora of the 28th August, and several subsequent nights, as observed at Lunenburg, Massachusetts, Lat. 42° 35′. By Professor WILLIAM B. ROGERS. With a Plate, . . . 90

7. Supplementary Remarks on the genus Galago. By ANDREW MURRAY, Edinburgh. With a Plate, . 99

REVIEWS:—

1. Christianity contrasted with Hindu Philosophy. By JAMES R. BALLANTYNE, LL.D., Professor of Moral Philosophy, and Principal of the Government College at Benares, ... 103

2. Popular Tables, arranged in a new form, giving information at sight for ascertaining, according to the Carlisle Table of Mortality, the value of Lifehold, Leasehold, and Church Property, Renewal Fines, &c., the Public Funds, Annual Average Price and Interest on Consols from 1731 to 1858; also, various interesting and useful Tables, equally adapted to the Office and

PAGE
the Library table. By CHARLES M. WILLICH, Actuary and Secretary to the University Life Assurance Society, ... 107

PROCEEDINGS OF SOCIETIES:—

British Association, ... 108
Botanical Society of Edinburgh, . . . 141

SCIENTIFIC INTELLIGENCE:—

BOTANY.

1. Distribution of Vegetable Species. 2. Remarks on the Botany of Japan, in its Relations to that of North America, and of other parts of the Northern Temperate Zone. 3. Vegetable Hybrids. 4. Vegetation of the Sea of Aral, . . . 157–163

ZOOLOGY.

5. Egg of Dinornis. 6. Polyps. 7. Remarks by Dr Daniel Wilson on a deformed fragmentary Skull, found in an ancient Quarry Cave at Jerusalem, . 164–166

GEOLOGY.

8. Extract of a Letter from the late Mr James Motley, Engineer to the Julia Hermina Coal-mines of Borneo. 9. On the Genus Graptolithus. 10. Exploration of British North America, . . . 166–172

PUBLICATIONS RECEIVED, ... 172

CONTENTS.

PAGE

1. On Gebel Haurân, its adjacent Districts, and the Eastern Desert of Syria, with Remarks on their Geography and Geology. By John Hogg, M.A., F.R.S., F.L.S., &c., ... 173

2. On the Application of certain Laws of Heat and Combustion to the Use and Economy of Fuel. Being the substance of a Paper read before the Aberdeen Philosophical Society, 11th February 1859. By Alexander D. Milne. Part II. . . 192

3. Notes on Californian Trees. By Andrew Murray, F.R.S.E. Part II. Four Plates, . . 205

4. On the Incorrectness of the Present Mode of Estimating the Mean Temperature in England. By James Stark, M.D., F.R.S.E., &c. &c., . . . 228

5. Remarks on the recent progress of Sanskrit Literature and Comparative Philology. By John Muir, D.C.L., 242

PAGE

6. Some Miscellaneous Observations on the Tadpole; on the Albumen of the Newly-laid Egg; on the Growth of Birds—their specific gravity; and on the Stomach of Fishes in relation to Digestion. By JOHN DAVY, M.D., F.R.SS. Lond. and Edin., . . 252

REVIEWS:—

1. The Plant-covering of the Earth; a Popular Exhibition of Botanical Geography for Well-informed Lovers of Nature, composed from the Newest Sources. By LUDWIG RUDOLPH.

Die Pflanzendecke der Erde Populäre Darstellung der Pflanzengeographie für gebildete Naturfreunde nach den neuesten und besten Quellen zasammengestellt und bearbeitet VON LUDWIG RUDOLPH, Zweite vermehrte Ausgabe, ... 271

2. On the Origin of Species by Means of Natural Selection, or the Preservation of Favoured Races in the Struggle for Life. By CHARLES DARWIN, M.A. . . 280

PROCEEDINGS OF SOCIETIES:—

British Association, ... 290

Royal Society of Edinburgh, ... 312

Botanical Society of Edinburgh, ... 333

SCIENTIFIC INTELLIGENCE:—

PAGE

BOTANY.

1. Flora of Ceylon, ... 338

GEOLOGY.

2. Post-Pliocene Drift, ... 339

MISCELLANEOUS.

3. Peculiar Sounds under Water. 4. Abstract of the Meteorological Register for 1859, kept at Arbroath, 340–343

PUBLICATIONS RECEIVED, ... 344

INDEX, ... 345

THE

EDINBURGH NEW

PHILOSOPHICAL JOURNAL.

Observations on the Genetic Cycle in Organic Nature, and particularly on the Relation between the different Forms of Alternation of Generations and the more Ordinary Modifications of the Reproductive Process. By GEORGE OGILVIE, M.D., Lecturer on the Institutes of Medicine in Marischal College and University, Aberdeen.*

§ 1. *Origin of Organic Beings.*

The time is not yet out of mind when the doctrine of spontaneous generation was the great point of discussion in the physiology of reproduction. Now that this question has been set at rest by evidence as conclusive as any of a negative kind can well be, the attention of physiologists is chiefly directed to the relation between derivation in the ordinary way from two parents, and that other mode of origin from a single pre-existing form, of the prevalence of which among the lower species additional evidence is continually brought before us. In this mode of origin, which has received various names from authors, such as *Gemmation*, *Homogenesis*, and *Monogenesis*, a portion of the body of the parent becomes the seat of a certain independent manifestation of vitality, whereby the plastic processes are so much intensified that, in the course of time, the part is converted into a distinct organism, capable of detachment from the parent, and fitted to maintain a separate existence. Such a detached gemma may be termed a free zooid, or phytoid. In the ordinary form of reproduction, again, that by the co-operation of the sexes, otherwise termed *Heterogenesis*, or *Digenesis*, a fusion takes place of two highly vitalized portions of the same or kindred organisms, and results in the formation of a fecundated germ, possessed henceforth of an independent vitality, endowed with a capacity for ultimately acquiring the structure characteristic of the species, and destined to be thrown on its own resources, by its extrusion from the protecting envelopes, as soon as its organisation is sufficiently advanced for this condition. In all but the very lowest forms of life, the conjugating algæ, a difference is observable between the two factors of embryonic life, which are recognised respectively as male and female, or as the spermatic and germinal elements.

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* Read before the British Association for the Advancement of Science, September 1859.

§ 2. *Relations of Ova and Gemmæ.*

It is strongly contended by some that there is such an incompatibility between these two modes of propagation, that, in proportion as any portion of the parenchyma of the parent is engaged in the one course, it is proportionally disabled for the other. This opinion is founded on these alleged peculiarities of the sexual elements in their mature condition; 1*st*, That singly they are not capable of any farther development, but very soon lose their vitality, and undergo decomposition; and 2*d*, That they both differ very remarkably in appearance from all self-developing foci of vital action, and, in particular, that the germinal element of animals, or the unimpregnated ovum, though it may occasionally present a general resemblance to a gemma or bud, is always to be distinguished from it, by containing in its interior a peculiar nucleated cell, the germinal vesicle. But some, at least, of these peculiarities can no longer be maintained as constant characters. Quatrefages states that he has seen segmentation take place, independently of impregnation, in the ova of Hermella and Unio, though no farther development follows. In regard to the structural characters, again, even putting aside the evidence concerning the germs of the viviparous aphides as somewhat discordant, though, certainly, on the whole, in favour of the essential identity of ova and gemmæ, the observations of Mr Lubbock on the agamic ova of Daphnia, and those of Mr Smith of Kew, Professor Braun of Berlin, Radlkofer, and others, on the unimpregnated ovules of Cœlobogyne, appear conclusive to establish that bodies elaborated side by side with the true germinal elements, and in some cases undistinguishable from them in appearance, may undergo development independently of impregnation; while those of Dzierzon and Siebold on the hive-bee, go to show that the very same germs may undergo evolution either with or without impregnation, developing, in the former case, a female, and in the latter a male progeny. We can hardly, therefore, as it would seem, avoid adopting Professor Owen's conclusion, that there is no *essential* difference between an ovum and a gemma, and that the one may pass into the other by insensible gradations. We may assume, perhaps, that up to a certain point, the development of the new focus of vital action may go on all the same for a gemma or an ovum; but that towards the period of maturation the changes which take place in the latter to fit it for impregnation cause such a tension, as it were, of its vitality, as is incompatible with its continuance in the majority of cases, unless re-invigorated by the access of the spermatic element.

§ 3. *Alternation of Generations.*

Propagation by gemmation has been regarded as perpetuating the individual rather than the species, the successive zooids or phytoids preserving more completely than the progeny of embryonic origin the characters of the parent stock; and it has been thought, too, that there is a tendency for the plastic power to wear out, in process of time, so that a recurrence of sexual generation at intervals is necessary to preserve the pristine vigour of the species.

However this may be, there is reason to believe that the more highly organized the species is, the more dependent it is on the frequent recurrence of sexual reproduction in the genetic cycle. At least, we find that in the lowest forms there may be a very prolonged pullulation of gemmæ, the sexual act recurring only at distant intervals, and in some cases not being as yet positively known to occur at all; while in the higher animals we meet with no obvious phenomena at all of the nature of gemmation. In those of the lower species in which both modes of propagation are well-marked features, we find that they have a tendency to succeed each other in a regular order, with corresponding differences in the immediate progeny, to which the term of alternation of generations has been applied; and this expression, though open to some objections, has come into very general use.

A complete parallelism, however, cannot be maintained for all the cases that go under this name; and as I am not aware of any systematic analysis having been made, to determine the nature of the differences, it is my object on the present occasion to bring forward certain distinctions which have impressed themselves on my mind as of fundamental importance, depending principally on the period in the life-history of the species, at which a process of gemmation is interpolated in the genetic cycle.

The gemmation sometimes occurs just before, and is, as it were, ancillary to sexual reproduction—sometimes it occurs after it, when it is subservient rather to the progress of development. In the former case, what may, on the whole, be considered as the most typical of the diverse forms belonging to the species, is still defective in having no proper organs of reproduction—a function which is vicariously performed by a set of gemmæ detached from it. The original stock is really neuter; but true sexes appear in these buds, after they have been transformed by a process of development into isolated zooids or phytoids. They may be considered as a highly individualized form of those organs which were wanting in the parent stock. Such organs constitute, at least, the essential part of their economy; and although, along with them, there may be present also others, more or less fully developed, for discharging functions, such as alimentation and locomotion, required by their status as free zooids, yet their great office is reproduction, and this end effected, their life speedily comes to a close. In this they contrast strikingly with the stock from which they were derived; for it is endowed with much greater permanence of life, frequently detaching during its period of vigour many successive swarms of sexual zooids, just as among the higher animals the same parent may develope many successive broods of young.

On the other hand, when the budding process occurs in the course of development, the gemmæ are detached from the immediate product of impregnation, while it is still in a rudimentary condition, comparable to the first stage in the evolution of the ovum of the higher animals. The germ-parent never itself attains to the full development of the species, but remains the whole term of its brief existence in a rudimentary state; but the progeny, which it buds off, acquire, in due course, the typical form, or at least give origin, mediately or immediately, to others which do so.

These two kinds of zooids, however, though differing so widely in their relations and structure—in the one case the primary products of impregnation, the precursors of the perfect form, and without sexual characters, in the other derivative and with distinct sexes—have yet this one point in common, that the great end of their existence is the multiplication of the race—an end to which the nutritive and animal functions are always subordinated. Such, indeed, is the occasional degradation or non-development of structure, that some zooids of both kinds might readily pass for mere egg-sacs or proliferous cysts.

§ 4. *Protomorphic Alternation.*

For the better distinction of these varieties of alternation, and for the purpose of bringing out more clearly what I consider to be their points of correspondence with phenomena occurring in the higher animals, I have found it convenient to divide the life-history of an organic being into three stages, all of which come out prominently in one form of alternation or other, while, as I shall presently endeavour to show, they are covertly represented even in those species, in which no phenomena of alternation are recognised. The first, or what I term the *Protomorphic* stage, is that which intervenes between the fecundation of the germ and the first appearance of the characteristic or typical organisation of the species; the second, or *Orthomorphic,* that which corresponds to the development and full perfection of this organisation; while the third, or *Gamomorphic,* is that of the formation or maturation of those structures in which the spermatic and germinal elements are generated, in preparation for another act of fecundation, as the commencement of a new genetic cycle.

In one of the forms of alternation just noticed, the interpolation of gemmation takes place in the protomorphic stage—that is, prior to that development, by which the features most characteristic of the species are gradually evolved. Of this we have an example in the case of the Trematode Entozoa, so often referred to by writers on the subject of alternation. In these animals the immediate product of the impregnated ovum is a free zooid, which never rises itself above a rudimentary condition, or acquires sexual organs, but which, by a process of asexual gemmation (*monogenesis*), ultimately originates others, which do attain to the typical character of the species, in the general organisation, and commonly also in the sexual relations, and then propagate in the manner of the higher orders.

For another illustration, we may turn to an allied family, the Cystic Entozoa, now known to be merely rudimentary forms of Cestoid worms. Their transformation into the latter is their most notable change, but, prior to it, they present us with a series of successive forms, all referable to the cystic phase. The typical form is the "Tænia-head," which is not the immediate product of impregnation, but is derived as a gemma from the primary cyst, into which the contents of the ovum are first developed. With great differences of detail, this general relation is to be traced in all the species. Thus, in the *Echinococcus hominis,* a vesicular mass is formed from the primary cyst, by the pullulation from its interior of secondary and tertiary structures of a like kind. Numerous gemmæ are developed from the last-formed cysts, having the general characters of "Tænia-heads." In another species or variety of *Echinococcus*, similar "Tænia-heads" are formed, in connection with vesicles budded off from the interior of the primary cyst, without the intermediate pullulation of other cysts. In *Cœnurus*, also, there is a formation of a multitude of "Tænia-heads" from the original cyst, only they are budded off from a special thickening of the lining membrane —not as in *Echinococcus*, from its whole interior.

The Echinodermata might also be cited in illustration, though they differ from the generality of cases of alternation, in the primary form budding off but a single secondary one.

As an example of protomorphic alternation in the vegetable kingdom, the case of the mosses may be referred to. Impregnation is now admitted to be as necessary a step in the reproduction of these as of any phanerogamic plants, but it is not the immediate precursor of the formation of an embryo. In mosses, the germinal element is represented by the central cell of the archegonium; and this, when fecundated by the spermatic filaments contained in the cellules of the antheridium, developes by endogenous formation a whole mass of cells, which, by a process of transformation in the course of growth, assumes eventually the form of the theca or capsule, with seta, calyptra, operculum, peristome, columella, and internal mass of dust-like spores. The spores in germination give rise to confervoid threads, which, after ramifying into a mass of tangled filaments—the protonema—send up here and there a leafy axis, bearing eventually, like the original one, antheridia and archegonia. In thus regarding the case of the mosses as parallel to that of the Trematoda, and illustrative of protomorphic alternation, of course I look upon the nascent axis as the true embryo, and the fully-developed moss as the typical form, and regard both the theca and the protonema as intermediate forms, no more represented in the higher plants, than the gregariniform zooids of distoma are in the higher grades of animal life.

§ 5. *Gamomorphic Alternation.*

The other form of alternation, before referred to, is that in which the process of gemmation is interpolated in what has been here termed the *gamomorphic stage, i.e.*, after the general acquisition of the typical conformation of the species, and in connection with the development of the organs which form the sexual elements. To this head I refer the alternation of polype and medusa forms, which is so common a feature in the life-history of the hydraform zoophytes, the normal or typical form being assumed to be that of the polype, and the medusa form being regarded simply as a highly individualized generative organ detached from the system of the polype. This view readily enough commends itself to our judgment in those species of *Laomedea*, &c., in which the medusiform zooids have so much the character of mere generative appendages, while the zoophyte condition stands prominently forward as the typical or orthomorphic phase, being represented by structures of greater permanence than the brood of minute and rudimentary medusoids which they throw off, and occasionally attaining considerable dimensions by the repeated pullulation of new polypes. But in the case of the Hood-eyed Medusæ, the *primâ facie* aspect of the case is entirely the other way; for not only are they themselves of much more conspicuous dimensions and elaborate organisation than the medusoids of the compound zoophytes just referred to, but the polype stock from which they spring is of such insignificant proportions, that it generally goes under the name of a larva, the resulting medusæ being regarded as the typical or perfect form of the species. Thus, Professor Owen remarks, "The medusiform ovigerous locomotive or distributive individual of the *Coryne* and *Campanularia geniculata* is evidently homologous with the polypiform ovigerous individual, which seems to nurse, as it were, the ova into 'planulæ' in the *Campanularia dichotoma*, and the nutritive gemmiparous polypiform individuals in all the compound Radiaries would seem, rather than the oviparous medusiform ones, to manifest the typical form of the species. . . . Superadd, however, distinct nutritive and circulating organs to the free moving ovigerous individual from the rooted polype, and prolong its existence, and it would then cease to have the ancillary character of a nurse to the ova of the fixed individuals, and would assume that of the perfected form of the species; and such, in fact, is the case with the larger gelatinous Radiaries, called *Medusæ.*"* Now, in so far as perfection means elaborateness of organisation, it is not, of course, to be denied that the medusa is in advance of the polype; but as regards the selection of the phase to be taken as the typical form of the species, I do not see how we can avoid these conclusions:—1*st*, That the bare-eyed medusoids are really homologues of the parts of reproduction, inasmuch as they pass by a continuous gradation into generative organs of the simplest kind and, 2*d*, that in so natural an order, the relative position assumed for the puny bare-eyed Medusæ must hold also for their portly brethren of the hood-eyed kind.

My limits prevent me giving anything like a detailed view of the declension referred to from free medusoids to simple tunicated ova attached to the body of the parent; but the following may be noticed as observable links in the series.† In *Campanularia dichotoma*, the medusoids are no longer free as in *C. geniculata;* they have also more of the polype-form, and remain during their brief period of life attached to the edge of the horny "ovigerous capsule," characteristic of the zoophyte, and there emit the ova or spermatozoa with which they are charged; after which they wither away like blossoms, to be succeeded by a new expansion. In *Campanularia lacerata*, the ovarian sac advances to the mouth of the capsule; but, instead of a bell-shaped envelope, becomes invested merely by a thick gelatinous coat. In *Sertularia* generally, the appendages, with somewhat of the medusoid conformation, mature and discharge their contents while still within the "ovigerous capsule." In *Cordylophora*, the only medusoid features presented either by the spermatic or the ovarian cyst, are the presence of a central tongue or columella to represent the proboscidiform mouth, and the existence of phlebenteric canals in its wall. In *Hydractinia,* we have the columella without the canals; and the cysts of some species of *Plumularia* and *Eudendrium* are, if possible, of still simpler structure, the latter containing but a single ovum. The progress of degradation reaches its maximum in the common *Hydra,* in which the large *Medusæ* of the "*Hydra tuba*" are represented only by spermatic and ovarian cysts, of the most rudimentary organisation, attached to the exterior of the polype. Closely allied species sometimes differ remarkably in this respect; and even in the same species there may be as great a diversity in the opposite sexes; thus in *Laomedea geniculata,* the ova are formed in free swimming medusoids; the spermatozoa in simple cysts permanently attached. Variations of the same kind occur also in the allied order of Physograda. Such variations, though perplexing to the systematic zoologist, are especially valuable to the physiologist, as indicating the true relations of the forms which occur in dimorphous species; and I think they are fully sufficient to bear us out in the conclusion, that both the bare-eyed and the hood-eyed Medusæ are to be considered as gamomorphic zooids, and the polype stock from which they sprang as the typical form in each case. In the one, the orthomorphic form is, as usual, the most conspicuous phase of the species, while in the other it is quite eclipsed by the resulting gamomorphic zooid, which is really a part of itself—a detached and overgrown organ of its own system.

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* Parthenogenesis, p. 12.

† See several papers by Dr T. S. Wright in previous numbers of this Journal.

As parallel cases, I would refer to the relation subsisting between the solitary and catenated *Salpæ,* which, as described first by Chamisso, may be regarded as the original basis of the doctrine of alternation, and of which Mr Huxley has since given us a most lucid and philosophical account—to the detachment of reproductive zooids, made up of caudal segments, budded off from some Annelida, as described by MM. Edwards and Quatrefages—and to the derivation from the "Tænia-head" of the *proglottides,* or cucurbitiform segments of the "body" of the tape-worm.

In the vegetable kingdom we have also a very parallel case in the reproduction of the ferns. In these plants, it is well known the sexual elements are not formed in connection with the conspicuous vegetative stem, but in minute derivative phytoids, termed prothallia, which are produced by the germination of the spores. The prothallia bear antheridia and archegonia; and the embryo, formed on impregnation from the central cell of one of the latter structures, grows up from the prothallium, which comes to have very much the appearance of the seed-leaf of the young shoot. The prothallia I regard as gamomorphic phytoids, parallel to the medusiform reproductive zooids of the Polypifera. Hence I feel obliged to dissent from the parallelism which Hoffmeister would establish between the reproductive process in ferns and mosses. This great authority regards as equivalent structures the prothallium of the former and the leafy axis of the latter, on the ground of their being the parts which bear the sexual organ; and argues from this a corresponding relation between the frondiferous stem of the fern and the seta and capsule of the moss, as the immediate products of impregnation in the two cases respectively. A comparison of objects of such *primâ facie* diversity—objects more unlike than even the large *Medusæ* and the ovarian cysts of the *Hydra*—ought not, I conceive, to be adopted, except on the most convincing evidence. But there is no such cogency in this case, on the admission of the general view which has now been advanced; for we have a readier solution of the difficulty, in assuming that the interpolation of an intermediate form occurs at a later stage of the genetic cycle in ferns than in mosses. For this view we have ample warrant in the analogy of the animal kingdom, where we find corresponding differences between the Cestoid and Trematode Entozoa, and between the latter and the Polypifera, among which, indeed, even nearly allied species differ in this matter of the interpolation of gemmation. Such a view, I submit, is a less tax on our powers of conception, than to regard the minute and fugitive capsule of the moss as the equivalent of the perennial and towering stem of the tree-fern. And it is to be borne in mind that the difference here is not merely one of a *primâ facie* kind. In some respects it increases the more we contemplate it; for it is clear, as Mr Jenner observes,* that the persistent character of the leafy axis of the moss, and its yielding, in perennial species, many successive sets of sporiferous capsules, assimilates it, quite independently of structural features, rather to the stem of the fern than to its prothallium, which is an organ even more evanescent than the capsule of the moss, its existence terminating when the embryo formed in it has begun to germinate.

§ 6. *Orthomorphic Alternation.*

In the intermediate period of the life-history of the species, that here termed *orthomorphic*, which intervenes between the appearance of the general typical character of the family and the maturation of sexual organs, gemmation, though perhaps a more frequent character than either in the incipient or terminal stages, rarely comes before us as a case of alternation of generations, in consequence of the gemmæ commonly remaining in adhesion to each other, so that their separate individuality is lost, and the whole aggregation passes as a single plant or animal. This is especially the characteristic arrangement in the vegetable kingdom, and in Polypifera and Polyzoa among animals. Where the gemmæ do become detached, however, the case may assume the aspect of a form of alternation, as we see strikingly exhibited in the propagation of the *Aphides*.

I may here briefly explain why I am disposed to refer the alternation of the *Aphides* to the commencement of this stage, rather than to the protomorphic. It is because the organisation has already acquired that partially advanced development characteristic of the larvæ of other insects, before the process of gemmation comes into play. We cannot say here that the primary product of impregnation buds off a set of embryos of a higher organisation; it is rather a larva—that is, a naked embryo—already so far organised on the insect type, that buds off a series of similar larvæ, the last only of which become perfect insects.

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* Edin. New Phil. Jour., New Series, Vol. III., p. 269. The occasional conversion of the fruit of the moss into a leafy shoot has been thought to indicate its analogy to the stem of the fern. Is it not rather a viviparous inflorescence, such as occurs at times in the higher plants?

§ 7. *Resumé of the Varieties of Alternation.*

On the grounds above stated, it becomes necessary to distinguish these three varieties of the so-called alternation of generations,—that is, of the alternation of gemmation with sexual reproduction :—

1. That in which the gemmation occurs in the protomorphic or germinal stage, prior to the appearance of the typical organisation ;
2. That in which it occurs in the gamomorphic or later stage of the life-history,—that is, in connection with the maturation of the reproductive organs ; and,
3. That in which it occurs in the orthomorphic or intermediate stage,—that is, during the manifestation of a more fully developed condition of the typical organisation, but prior to the maturation of the sexual organs.

The contrast lies principally between the two former varieties. They cannot, indeed, be identified or confounded, as they are by many authors, without losing sight of two important points of difference ;—

1. In the budding-stock, which in gamomorphic alternation has both a higher organisation and a greater permanence of life than are possessed by the protomorphic zooid or germ-parent of the typical form ;
2. In the off-sets or concluding links of the respective series, which have really nothing in common but the single point of sexual completeness, the medusoids, prothallia, and other gamomorphic forms being generally of the most rudimentary structure.

Hence, in the one case, we speak of the typical organism and its germ-like matrix ; in the other, of the typical organism and its sexual offset. Both the matrix and the offset may assume, indeed, the form of independent beings, but their life is always transitory and provisional, having reference to one common end,—the multiplication of the race,—though by different means. The great function of the germinal or protomorphic zooids is the evolution of the more perfect embryos, of which they serve as budding-stocks; that of the sexual or gamomorphic zooids is the development of ova and spermatozoa. These ends accomplished, their vitality ceases, while the typical organism, the offspring of the former class, or the parent stock of the latter, as the case may be, has a much more permanent duration, and may go on for a long time in perfect vigour, sending off crop after crop of ova, or of sexual gemmæ, according to its mode of propagation.

The distinctness of these varieties of alternation is further shown by their occasional co-existence in the same species, as in some Cestoid worms, and perhaps in a more latent form in the case of the Polyzoa* and of some Annelida. These, however, are exceptional cases, for it would appear that organisms which are propagated by protomorphic gemmation do not ordinarily throw off sexual zooids, and that species in which the latter phenomenon occurs do not usually furnish instances of proembryonic forms.

§ 8. *Continued Pullulation in the same stage.*

The regularity in the alternation of free zooids with true embryos is frequently obscured in nature by the intervention of a process of *pullulation*, or budding off of like forms, in continued succession, at some particular stage in the life-history of the species, so that sexual zooids recur only at intervals, separated by periods during which a series of neuter forms occur of the same general character, if not all absolutely alike. In some cases, the number of interpolated links appears to be fixed; but in general it is variable, and frequently the recurrence of the sexual form which closes the series seems to depend on circumstances, the true ova being commonly formed on the approach of winter, or other conditions adverse to the continuation of active vitality.

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* Allman's British Fresh-water Polyzoa (Ray Soc.), p. 41.

A course of pullulation may be thus interpolated at any stage. It is met with in the germinal stage in the case of the Trematoda, and in the gamomorphic or sexual stage in a few medusoids; but more commonly it occurs in the orthomorphic stage, being interposed between the first appearance of the typical characters, and the development of the structures which originate the sexual elements. In fact, the orthomorphic gemmation, just noticed as one form of alternation, almost always runs on into a continued course of pullulation, the result being either a swarm of free zooids, as in the case of the *Aphides*, or else a composite structure, like the leafy stem of a plant, or the polypidom of a zoophyte. The latter alternative is the more common; for the tendency of the gemmæ, in most cases of continued pullulation, is to remain during their whole term of life in connection with the parent stock, either directly, or through the medium of their predecessors in the series of offshoots.*

§ 9. *Protomorphic Alternation in relation to Embryogeny.*

Though the well-marked cases of alternation, due to the evolution of protomorphic zooids, are confined to a few of the lower orders, a certain *nisus* or tendency in this direction—a fresh start, as it were, in the course of germinal development—may be traced with more or less distinctness in all cases of embryogeny, as in all instances there is formed first a cellular germ-mass, from one point of which there is subsequently developed a new axis of embryonic growth.

The embryo, in short, may be said to be budded off from the primordial germ-mass, much as the larval distoma is from the gregariniform product of the Trematode ovum. There are, however, two points of diversity. In normal development, the germ-mass gives rise only to a single embryo, and no separation takes place between them. The later growth appears simply as a more advanced state of the former, which wastes away, *pari passu*, with the growth of the embryo, becoming a mere appendage of the latter, or disappearing altogether. In alternation or metagenesis, again, the immediate product of the ovum gives rise to *numerous* gemmæ, every one of which may acquire the characters of a typical individual of the species; and we find that these gemmæ generally become completely separated from their germ-parent, and assume the form of independent organisms. But although the detachment of the later growth, and its multiplication, give an apparent distinctness to the cases in which they occur, there are yet phenomena of an intermediate kind, which indicate a certain community of nature between them. Such are the following:—

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* In the tabular views of the Genetic Cycle, given at the end of this article, such continued pullulation—as being only an occasional phenomenon—is printed in a smaller type.

1. The duplication, in whole or part, of the embryonic axis, as an occasional abnormality, even in the higher species, resulting in the formation of a double monster.

In the eggs of the pike, according to M. Lereboullet, "the formation of these monsters may be determined at pleasure, by placing the eggs in unfavourable conditions for development." In this case the blastodermic ridge forms on its surface two tubercles instead of one, and from each of these an embryonic fillet is produced, the further development of which gives rise to double embryos of various kinds.*

2. The regular formation of a double embryo from the ovum, in the case of the Polyzoa.

Here the immediate product is a ciliated germ-mass, like an infusorial animalcule, from a protrusion of which, according to Professor Allman, a pair of polypes are budded off in succession, the process presenting, as he observes, some remarkable analogies, tending to bring the whole process of gemmation and generation within the domain of the so-called "law of alternation of generations,"† though neither the two first-formed gemmæ, nor those which afterwards pullulate from them, ever become detached, while the original germ-mass becomes as completely reduced to the condition of a mere appendage of the structures derived from it, as in the case of the ovum of any vertebrated animal.

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* An. Nat. Hist., 2d Series, xvi., 49.

† British Fresh-water Polyzoa (Ray Soc.), pp. 41, 33, 34.

3. The variable character of the gemmation of Tænia-heads in the cystic Entozoa—solitary in the *Cysticercus*, but multiple in the case of the *Cœnurus* and *Echinococcus.* *

4. The co-existence among the Echinodermata of cases resembling ordinary embryogeny, or the metamorphosis of insects, as in *Echinaster* or *Holothuria*, with others, constituting the majority of the class, in which the embryo, though still solitary, stands out as a distinct structure from the so-called larva, and has in so far the character of a derivative zooid.

The case, therefore, seems to stand thus. Embryonic gemmation may be said to occur in all cases, though in the higher animals only in a latent form; while in the lower species, it is so exaggerated as to acquire a wholly new character. So long as the exaggeration is merely in its distinctness, or in the more complete detachment of the gemma, the affinity of the process to the normal course of embryogeny is sufficiently apparent (as in the Echinodermata). Even when a new element of discrepancy is introduced by a multiple gemmation, we can still find a parallel in the embryogeny of the higher animals, though now only as an occasional abnormality. But when the breach is yet further widened by one or more repetitions of the process of gemmation, we have results so totally unlike the ordinary course of reproduction in the majority of animals, that it is with some difficulty we can realize any community between them.

§ 10. *Gamomorphic Alternation in relation to Sexual Maturation.*

As the appearance of a new centre of organisation in the cellular germ-mass may stand in the higher species as a representation of protomorphic alternation, so to the contrasted form—marked by the formation of sexual or gamomorphic zooids—may we trace a certain correspondence in the maturation of the reproductive organs. Such a correspondence is suggested in particular by the following considerations:—

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* An argument of all the greater cogency if Cœnurus be, as Siebold contends, a mere variety of Cysticercus. Even in admitted forms of Cysticercus, however, such multiple gemmation of Tænia-heads has been observed.

1. The periodicity and lateness of development of the organs of reproduction in most species, and their greater or less independence of the rest of the system in some cases. This is so much the case in the Polyzoa, that, in the opinion of competent judges, they hold the position rather of derivative gemmæ, than of mere organs of the particular polypes, in connection with which they are developed. Thus, Professor Allman remarks, "If the formation of the ovary be attended to, it will be seen that this body is developed at a later period from the walls of the original sac-like embryo, which have undergone slight changes, and have become the endocyst of the more mature polyzoon, and it will be at once perceived that this development of the ovary takes place in a way which may obviously be compared with the formation of a bud; that—at least in *Alcyonella*—it occupies exactly the position in certain cells that the buds destined to become polypides [polypes] do in others, and that, at an early stage of polypide and ovary, it is scarcely possible to distinguish one from the other; so that the idea is immediately suggested, that the body here called *ovary* is itself a distinct zooid, in which the whole organisation becomes so completely subordinate to the reproductive function as to be entirely masked and apparently replaced by the generative organs." On similar grounds he argues, that the spermatic organ "may perhaps be more correctly considered, like the ovary, as a distinct sexual bud, having the generative system so enormously predominant as to overrule and replace all the rest of the organisation; this bud, like the ovary-bud, being also unisexual, but with a male function."

2. The transition in certain families, as the Polypifera, and even among closely allied species, from cases in which the reproductive organs are integral parts of the system and of very simple structure, to others in which they occur in detached zooids, having the character of distinct and well-organised animals.

3. The accidental nature of the characters which principally

distinguish these zooids from the ordinary organs of reproduction—detachment, and complexity of organisation.

The transition just noticed in the order of Polypifera is sufficient to show that differences in these points cannot be allowed any weight in a question of this kind. In regard particularly to what may be termed the adventitious organisation of the reproductive zooids, as compared with mere organs fulfilling the same function, this conclusion is strengthened by the contrast of a phenomenon of an opposite kind,—the degradation of individuals in certain species to the position of mere sexual mechanisms, these individuals being truly distinct from their origin—not mere zooids budded off from other forms, but animals developed independently from impregnated ova. The males of certain Rotifera, and still more those in the Cirripedia which are termed parasitic or complementary, are examples in point. In the structures now contrasted, we have examples of the two extremes of organisation; in the one case we have a *member* organised above par, so as to simulate a complete animal; in the other we have a true *animal*, so far below par in its structural development, as to resemble a mere organ. The contrast shows in a striking way that the suppression of normal parts in an animal, or the development of adventitious structures in connection with any particular organ, are not of essential importance in determining what has been termed by some authors "zoological individuality."

The other character—that of detachment—hinges on the proportionate development of the *somatic* life, that is, the life of the body as one whole, and the more or less independent life of its several organs or what we may term the *topical* or regional life. In the higher animals, the special actions of the several organs are as completely subordinated to that of the body as a whole, as are the powers of local corporations to the central government in any well-ordered state; yet there still remains sufficient evidence of the real existence of a distinct topical life. The antlers of the deer, and the hairs and teeth of animals generally, furnish well-marked illustrations of it. The first set of teeth, for instance, are formed, each in its own capsule, by a process of local growth, quite independent of that of the neighbouring tissues, nay, in so far opposed to it, that, at a certain stage of development, the integuments of the gum are partially disintegrated to allow of their eruption. A tooth thus generated by independent growth, sometime after attaining maturity, undergoes a process of decay ending in its ultimate removal, when a new tooth, of the second dentition, takes its place by a similar process of local development. In its turn this tooth also is shed, and though in most species it has no successor, yet in a few there is a constant succession during the whole lifetime of the animal. Such also is the case with the growth of the hair in all species. Hence in such local formations as teeth, hair, &c., we have, both in the way in which they are marked off from the neighbouring parts, and in this succession of growth, maturation, and decay—repeated again and again, and epitomizing, as it were, the life of the animal on which they grow—evidence of a vitality quite as defined perhaps in itself as that presented by the free zooids of the lower species, though the functional dependence on the common circulation and the mechanical bond of a common integument prevent their exhibiting the more obvious phenomena of a separate life. But as we descend in the scale of organisation, we come to species where, from the absence of centralising influences, the several organs—which are possessed of a vitality, less energetic perhaps, but more enduring than in the higher—become emancipated, as it were, from the control of the general system, and appear as zooids, that is, in the guise of independent beings, rather than as integral parts of the same animal—suggesting the similitude of the feudal system of the middle ages, or of a loose confederation of Indian tribes, rather than of a well-ordered polity of our own day. And though the proper organs of reproduction, from their partial independence even in the higher animals, seem, as we might expect, to manifest most clearly this emancipation from the controlling influence of somatic life, yet it is seen very distinctly in others also, as, for instance, in the peculiarly modified tentacle of the *Argonauta*, which, when filled with spermatic fluid, is detached from the body, and finds its way spontaneously to the female, for the purpose of impregnation.

4. In the vegetable kingdom the correspondence of the archegonia formed in the prothallia or detached reproductive phytoids of ferns, to the intra-ovular structures of flowering plants, furnishes also an analogical argument of great weight in support of an essential community of nature between the proper organs of reproduction, and all such isolated gamomorphic forms, whether of the vegetable or the animal kingdom. This correspondence has been most satisfactorily traced by Hoffmeister and others, through the intermediate orders of Lycopodiaceæ, Marsileaceæ, and Coniferæ; but I need not make farther allusion to a subject on which I have nothing new to bring forward, and which, in any case, could not be fairly treated in the limits of this paper.

It formed part of my original plan, to make a few observations on the relations of metamorphosis to alternation or metagenesis, and to follow up these general statements with some remarks on the principal modifications of the reproductive process in the leading groups of both kingdoms of nature, with the view of showing how far the protomorphic, orthomorphic, and gamomorphic stages are represented both in the alternating and non-alternating species; but the extent of my first draught of the subject has shown me how impracticable this would be within the limits of such an article as the present. In the meantime, therefore, I must content myself with such indications of these relations as are suggested by the annexed tabular views to which I have had occasion to direct attention in the course of this paper.*

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* It is only since these remarks were sent to press that I have seen Radlkofer's observations on the function of reproduction in the animal and vegetable kingdoms. In the latter, this author distinguishes very clearly the varieties of alternation here termed Protomorphic and Gamomorphic, but in animals he seems only to recognise the first, so far, at least, as I can understand the translation of his paper in the "Annals of Natural History" (2d series, vol. xx., pp. 241, 344, 439).

TABLE I.

Table I.

GENETIC CYCLE IN ANIMALS.

PROTOMORPHIC STAGE.

Primary Development from the Ovum,

ALTERNATION OF GENERATIONS.	ORDINARY REPRODUCTION.
of a Protomorphic Zooid,—*e.g.* in *Echinodermata*—of *Auricularia*, *Bipinnaria*, *Pluteus*; in *Cestoidea*—of Cystic Forms; in *Trematoda*—of Infusorian, and Gregarinic Forms.	of the Cellular Germ-Mass or "Mulberry Body."
Pullulation of Derivative Protomorphic Zooids, as in *Echinococcus*, some *Trematoda*, &c.	———

Formation of "Embryo-Buds," e.g.,

ALTERNATION OF GENERATIONS.	ORDINARY REPRODUCTION.
of the Disc of the *Echinodermata*, or the Cercariform Embryos of the *Trematoda*.	of the "Primitive Trace" of Embryonic Organization.

ORTHOMORPHIC STAGE.

Development of the Typical Form,

ALTERNATION OF GENERATIONS.	ORDINARY REPRODUCTION.
as, in *Echinodermata*, of the Starfish or Sea Urchin; in *Cestoidea*, of the "Tænia-head;" in *Trematoda*, of the Cercariform Larva and the *Distoma*.	through the successive phases of Embryogeny; (Metamorphosis in the case of Larvæ or Naked Embryos, as in some *Crustacea*, *Insects*, and *Batrachia*.)
Pullulation in *Polypifera* and *Polyzoa*, of a series of Polypes cohering as a Polypidom; in *Aphis*, of successive swarms of free Larvæ, like the original.	———

Formation of "Brood-Stocks," e.g.,

ALTERNATION OF GENERATIONS.	ORDINARY REPRODUCTION.
The "Gonophores and Blasto-styles" of the *Polypifera*; The "Stolons" of the *Tunicata*.	The Tissue of the immature Ovarian and Spermatic Glands.

GAMOMORPHIC STAGE.

Development of the proper Reproductive Structures,

ALTERNATION OF GENERATIONS.	ORDINARY REPRODUCTION.
As in *Polypifera*, of Medusiform Zooids in *Salpa*, of the Catenated Form; in *Annelida*, of Caudal Zooids; in *Cestoidea*, of Proglottides.	as of Ovarian and Spermatic Follicles.
Pullulation of Secondary Medusoids in *Sarsia* and some other Species.	———

Formation of the Reproductive Elements, i.e., Ova and Cells with Spermatozoa.

FECUNDATION OF THE OVUM.

Table II.

GENETIC CYCLE IN PLANTS.

<table>
<tr><th>CRYPTOGAMIA.</th><th>PHANEROGAMIA.</th></tr>
<tr><td colspan="2">PROTOMORPHIC STAGE.</td></tr>
<tr><td colspan="2">Primary Development from the Fecundated Germ.</td></tr>
<tr><td>in <i>Hepaticæ</i> and <i>Mosses</i>, of the Theca, and Spores, which in the latter germinate into Protonemic Filaments</td><td>of the Cellular Pro-embryo or Suspensor.</td></tr>
<tr><td>Pullulation of Derivative Protonemic Filaments;</td><td>Quaternate division of the Suspensor in <i>Coniferæ</i>.</td></tr>
<tr><td colspan="2">Formation of "Embryo-Buds," viz.:</td></tr>
<tr><td>the Gemma of the leafy axis of <i>Mosses</i>;</td><td>the Embryonic Cellular Mass.</td></tr>
<tr><td colspan="2">ORTHOMORPHIC STAGE.</td></tr>
<tr><td colspan="2">Development of the Typical Form or Vegetative Axis of the</td></tr>
<tr><td><i>Moss</i>, <i>Fern</i>, or</td><td>Flowering Plant.</td></tr>
<tr><td colspan="2">Pullulation of Successive Shoots, generally remaining in adhesion, but sometimes developed from deciduous bulbs.</td></tr>
<tr><td colspan="2">Formation of "Brood-Stocks," viz.:</td></tr>
<tr><td>Perichætia of <i>Mosses</i>,
Sporangia and Spores of <i>Ferns</i>;</td><td>The Floral Organs.</td></tr>
<tr><td colspan="2">GAMOMORPHIC STAGE.</td></tr>
<tr><td colspan="2">Development of the proper Reproductive Structures, viz.:</td></tr>
<tr><td>in <i>Ferns</i>, of Prothallial Phytoids;</td><td>of Ovules and Pollen-grains.</td></tr>
<tr><td>Pullulation of occasional Secondary Prothallia in <i>Ferns</i>;</td><td>and of the "albuminous bodies" in <i>Coniferæ</i>.</td></tr>
<tr><td colspan="2">Formation of the Reproductive Elements, viz.</td></tr>
<tr><td>of Archegonia, with Germ-cells
and Antheridia, with Cellules containing Antherozoids</td><td>of Embryo-sacs with Germinal Bodies
and Pollen-tubes with Fovilla
(or in some <i>Coniferæ</i>, with Secondary Cellules).</td></tr>
<tr><td colspan="2">FECUNDATION.</td></tr>
</table>

Table III.

PERIODS OF INTERPOLATION OF GEMMATION
In the Genetic Cycle.

Protomorphic.	Orthomorphic.	Gamomorphic.
Mosses and Hepaticæ	Plants of all Classes.	Ferns and Equiseta
	Polypifera	Polypifera
Echinodermata		
(Polyzoa)	Polyzoa	(Polyzoa) Salpæ
Trematoda		
Cestoidea		Cestoidea
(Annelida)		Annelida (*Syllis*, &c.)
	Aphides	

Gemmation is exceptional, in any stage, among the higher Articulata and Mollusca, and is unknown as a normal arrangement among Vertebrata.

Table IV.

RESTING PERIODS IN THE GENETIC CYCLE.

Mosses and Hepaticæ *protospores*	In the middle of the Protomorphic stage.
Ferns and Equiseta *gamospores*	Between the Orthomorphic and Gamomorphic.
Phanerogamia *seeds*	Between the Protomorphic (embryogeny) and the Orthomorphic (vegetation commencing with germination).

Coniferæ present also another resting period, in the middle of the Gamomorphic stage (during the maturation of the fruit).

Animals in general, *eggs* or *ova*,	Between the Gamomorphic (maturation of the reproductive organs) and the Protomorphic (embryogeny).
Insects, Trematoda, &c.	Have also a resting period (*pupa* or *encysted state*) during their metamorphosis, in the early part of the Orthomorphic stage.

Mammalia have no obvious resting period, the mature ova requiring immediate fecundation, which is at once followed by segmentation and the development of the embryo.

The bodies which, under the names of *statoblasts, bulbs, resting spores*, &c., perform the part of eggs or seeds in some species of animals and plants, appear occasionally to be gemmæ, which may be termed *accessory*, as lying out of the direct genetic cycle.

Peruvian Gleanings. By Dr ARCHIBALD SMITH.

I shall first notice the supposed osteological type of wormian bones in all the crania of the Peruvian Indian race, alluded to by Daniel Wilson, LL.D., in the "Edinburgh New Philosophical Journal, New Series," Vol. vii. No. 1. Finding, upon due inquiry at the Medical College at Lima, that neither its deacon nor professors could give me the least information on this subject, I applied to Dr Lorenti, one of the best authorities in Peru on such subjects, and he at once assured me that Tshudi's statement is utterly untrue. But, further to satisfy myself on this point, I went to the Museum, and saw there five native Indian skulls from ancient tombs, in which the sutures were visible. In mummies and heads covered with integuments and hair, the sutures were not accessible: there is one mummy with a finely formed head worthy of a Grecian philosopher. But of the five bare crania, only one showed signs of a wormian bone at all; another skull was so compressed on the forehead that the bulk of the brain must have been in life pressed back on the occipital region. The same artificial shape of skull was also pointed out to me in one of the encased mummies; but these were evidently exceptional specimens. The super-occipital or inter-parietal wormian bone represented by Dr Wilson, on the authority of Dr Tshudi, as characteristic of *all the skulls of the Peruvian Indian race,* is not even traceable in any one of the five skulls in the Lima Museum. Besides, the skull from the ruins of Pachacamac, or the Temple of the Sun, seven leagues to the south of this capital, which was deposited by me in the Edinburgh Museum in the time of Professor Forbes, and to which, last year, I drew Professor Allman's attention, has no such peculiarity; neither have two skulls from the Chinchas, in the possession of Professor Simpson, the osteological peculiarity in question, and therefore none such can be said to be typical *of the Peruvians* as a race.*

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* Since writing the above, I have been introduced to Dr Charles Scherza, of the Imperial Austrian frigate "Novara." I directed his attention to Dr Tshudi's statement, and since then I have seen him on his return from the

An ethnological inquiry of not less interest than the craniological one, is, I think, the oriental origin of the ancient Peruvians.

In a town on the coast of Peru, and in the province of Lambayeque, called "Eten," there has existed from time immemorial an isolated community, that never liked to see strangers among them, nor to intermarry beyond their own border. They speak Spanish like the other natives of the coast, but also use a special language not understood by other Indians or inhabitants of Peru. It would not be allowable, perhaps, on the grounds of language alone, to conclude that the people of Eten were of different origin from other Peruvians. The Indians of Cusco, in the south, are not at this day well understood by the Indians of Cajamarca in the north; yet both these populations radically speak one Quichua language. In the same way, a native of the Isle of Man and of Skye in the Hebrides, though speaking radically the same Celtic tongue, do not easily comprehend each other in colloquial intercourse. But notwithstanding the speciality of a speech hitherto unique in Peru, the inhabitants of Eten have no physical peculiarities to distinguish them from the other native Indians of the coast; and accident has at length revealed the source of the unknown language of Eten. Among the Chinese lately introduced to Peru to occupy the place of the emancipated negro, it is observable, that natives of different remote provinces of that great Eastern empire speak an idiom often differing so much, as to make them unable to converse in Chinese; but it has so happened, that some of them having come in contact with a native of Eten, they could understand each other. And thus the special language of Eten is in reality a Chinese dialect, and therefore the Peruvian Etenians are of Chinese origin. The fact appears to be perfectly well attested, and is here believed by people of the best information on the subject. I have, the other day, seen in the family of Don Juan Rodrigues, who was the first to introduce Chinese labourers into Peru in

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ancient Temple of Pachacamac, where he excavated skulls from the tombs. He assures me that he has inspected at least fifty crania, and that none of them presented the characteristics of a super-occipital or inter-parietal wormian bone. He has six fine specimens to speak for the Inca race in Europe.

the years 1849–50, an Indian girl from the neighbourhood of Eten, and a Chinese from the neighbourhood of Pekin, so very like in features that I took them for brother and sister. I often amuse myself by contrasting the different countenances of the Chinese of different provinces of China, when, on pay-days, about eighty of them employed in paving the streets of Lima meet at Mr Rodrigues' office. This gentleman tells me that the greater part of the Chinese imported to Peru are from the north of China, especially from Shanghae, Amoy, Loting, and some also from Canton, Macao, and other places. Those with high cheek-bones and obliquely set eyes are said to be principally from the interior of China, and owe their special physiognomy to a mixture of the Tartar blood. But the greater part of the Chinese introduced into Peru from the above-named districts in China are, like our native Peruvians, of Inca race, without this cast of countenance; and I observe that those who have it are often of shorter stature than others of their countrymen. I have learned from landed proprietors who employ the Chinese on their estates, that they bury their dead with provisions for a future journey beyond the grave to their native land, much according to the practice of the ancient Peruvians. Mr Rodrigues further tells me, that they also use in China religious images very similar to those found in the ancient tumuli and tombs of Peru; and altogether, it is impossible to see them among our Indians, without being struck by the strong family likeness between the two people. In taking leave of this subject, I may mention, as a remarkable coincidence, that the Peruvian skull from Pachacamac in the Edinburgh Museum was there marked as a Malay specimen, a mistake no doubt originating in certain points of resemblance, though there could not be a doubt of its being Peruvian, as I took the precaution of writing in my own hand, on the frontal bone, before presenting it to the Museum.

There is another point to which I would here advert, not because it appears to me of transcendent importance in itself, but as a curious instance of the facility with which even men of science give credence to what is rare and wonderful, rather than to what is probable—I mean the assertion that cats, carried to the elevation of 13,000 feet, invariably sink, after being seized with very singular shocks of tetanus. This supposed fact I have seen quoted, upon the authority of Dr Tshudi, by no less a philosopher than Baron Von Humboldt. Now, since my return to this country a few months ago, I have taken pains to ascertain whether the alleged fact is true, or merely a special incident of travel greatly exaggerated and expressed as a general fact. The result is, that at Tuctococha, the mines of Dr Maclean, only ten minutes' walk beneath the snow-line of the Western Cordillera, the central Andine range of Peru, and far above 13,000 feet, cats live easily. In the important city of Puno, in South Peru, at the elevation of 13,000 feet, there are probably as many cats as there are huts or houses. In the province of Lampa, and department of Puno, is situated one of the loftiest mountains of the Cordillera, and which is called Pomaci. Near the summit of this snowy Andine peak there are mines and miners' huts within the region of perpetual snow; and in every one of which there is a cat, with this speciality, that all these cats are black. This peculiarity I quote on the authority of General San Roman, the present Minister of War in this country, and proprietor of silver mines in the summit of Pomaci. Mr Basagoitea likewise, at present Chief of the Custom-House department at Callao, and lately Guano Commissioner in England, is a native of Puno, and he assures me that not only in the city of Puno and neighbourhood, but in much higher stations, as Huallata, Apo, Rumihuasi, and Crucero a small Indian hamlet on the confines of Peru and Bolivia, where it is so cold that the water is carried as ice in baskets, the cat is a constant inmate of the Indian's hut. Dr Destruge of Guayaquil, and Dr Espinoa of Quito, have personally assured me that cats inhabit the Indian huts on the Hacienda del Pedregal in the Chimborazo, at the elevation of the snow-line. And to conclude all that I would here advance on this topic, I may say that Dr Lorenti, a well-known and highly esteemed man of science, who has travelled over a great part of the Andes, assures me, that wherever he found an Indian home, there he found the cat domesticated. It only remains for me to say, that in this country no one would for a moment distrust the witnesses I have named, and that their evidence in this matter is fully ratified by the concurrent testimony of all the miners and Deputies of Congress from the Sierra, &c., with whom I have made it a subject of conversation, as a point interesting to European men of science.

On the Goitre and Cretinism in the Cordillera.

I have seen it stated that the highest elevations of these diseases on the Cordillera is 14,100 feet. One would think that such precision of statement should be grounded upon exact observation. Before leaving Edinburgh for Peru in November last, I had an opportunity of expressing my doubts on the subject to the author of that valuable work —the Physical Atlas. With the frankness and candour becoming his position in science, I found him most willing to receive the few hints which my long experience of Peru authorized me to offer on this topic. I therefore conclude, that the brief notice I can offer in illustration of the habitat of goitre and cretinism in our Andine climates may not be altogether useless in the present state of European knowledge on the subject. Goitre may be met with at the specified elevation of 14,100 feet, or at 15,000 feet, but only in travellers or visitors at the mines, &c., from the valleys where the disease is endemic. In all the inter-Cordillera warm valleys of Peru, looking eastward, goitre more or less exists; and the usual mode of getting rid of it in recent cases is, to abandon the warm climate of the sheltered valley for the cool table lands or cold heights of the Cordillera. In the region of the Andes, where cold prevails all the year through, goitre is never endemic; and cretinism is very rare in Peru, even in those parts where goitre abounds. Thus, in the valleys of Cusco and Huanuco, those affected with goitre are generally as acute in mind as those that are free of this malady. I understand the same is generally the case in Sucre, the capital of Bolivia, where goitre is common in some districts and not in others—a difference ascribed to the quality of the water. Dr Lorenti, who long resided in Huancayo, says that goitre contracted there, is removed by a change to the cooler climate of Juaja, a few leagues distant; that the town of Concepcion, five leagues from Juaja, has goitre, whereas La Geronimo, only one league to the south of Concepcion, has it not—an immunity ascribed to the good water of the latter locality. In the warmer and lower districts to the east of Juaja, and also in the mild climates of Andamayllas, there are very largely developed goitres; and those affected by them are, by Dr Lorenti's account, often dull and stupid, and their children generally deaf and dumb, and are called "Upas" or "Opas," in the Indian Quichua tongue. But of all parts of Peru the remote province of Patas is the most infected with goitre, and even cretinism. This province, more than any other part of the Sierra of Peru, consists of uneven broken ground—successions of high hills and deep hot valleys, in which the sugar-cane and tropical fruits are freely produced. The town of Patas, the ancient capital of the province of the same name, is a mining place, from which silver, gold, and gold-washings are exported. The temperature of this auriferous town is warm throughout the year; and though it rains abundantly from October to April, during the dry season it is entirely free from frost. Its population is estimated at 150. The town rises on each side, from the bottom of the valley, like an amphitheatre, and is divided by a small ravine into two parts. On one side of this natural boundary almost all are said to have goitre, many among them deaf and dumb, and also idiotic and ill-shaped, as well as ugly. On the opposite side of the ravine or rivulet, the goitre is not only less in size, but much less frequent. On the side where the malady abounds, the people drink spring-water; while on the side that it prevails least, they drink from running water. At a distance of one league from the town there is a supply of brackish water, and those who drink of it are free from goitre, and it is a cure for those afflicted with it. The above account I repeat on the authority of Mr and Mrs Marcos,—the governor and his lady,—of the district of the Marañon de Conchucos. Two months past I attended this lady at the Italian Hotel in Lima, and cured her of endemic goitre, which she had contracted during eight months' residence in Patas, where her husband had mines. Finding that both she and her husband and their daughter (ten years old) became affected with goitre, they abandoned their mines, and retired to the colder regions, where Mr Marcos and his child recovered, and the goitre became much reduced in herself. I should from all these facts conclude, that in the Andine valleys of Peru goitre is never endemic above an elevation of from 9000 to 10,000 feet; and that if accidentally met with in the Cordillera, it must have been contracted elsewhere. I may add on this subject information received by me regarding other parts of the American Continent: for example, Dr Dertruge (a most competent authority, as one of the medical attendants of the army of Bolivar in Venezuela, New Grenada, and Ecuador) tells me that he has seen some cases of goitre, and even of cretinism, in the warm district of Riobamba, and in the mild climates of the Quebradas, or glens of the hill lands of Quenca, less elevated than that of Quito; but that in New Grenada he has seen goitre of so enormous a size as nearly to conceal the face, and also cases of cretinism, especially in the provinces of Locovio, Marequita, and Ocana, but only in warm climates, never in the Cordillera, where it appears incapable of development. Dr Lorenti confirms these statements.

On the Vestiges of Extinct Glaciers in the Lake Districts of Cumberland and Westmoreland. PART I. By EDWARD HULL, A.B., F.G.S. With two Plates.

In the following pages I propose to describe the principal effects referable to glacial agency along the southern watershed of the range of mountains stretching from Bowfell on the west to High Street on the east; reserving for, I hope, a future occasion, the like phenomena of the northern slopes.

This primary line of watershed passes across most of the highest points of the central chain along the limits of the two shires, and is a serious obstacle to intercommunication between the northern and southern districts, the lowest of the passes by which it is crossed—Dunmail Raise, at the head of Grasmere, being 725 feet above the sea. This watershed was once the *snowshed* of two systems of glaciers, which, flowing in opposite directions, drained the snowfields of Scawfell, Bowfell, Helvellyn, Fairfield, and High Street, with their accompanying heights.*

The Highlands of Britain and Ireland are generally recognised as having been the seats of glaciers during the Postpliocene Period of the Northern Drift; but the mountains and valleys of North Wales have alone been treated in a systematic manner by more than one author,† while the probably no less interesting regions of Cumberland and Westmoreland, the Killarney mountains in Ireland, and the Scottish Highlands, in all of which the traces of extinct glaciers are remarkably distinct, are almost virgin soil, as far as this part of their history is concerned.

In the case of the Lake District of England, observers are still sadly in want of maps on a sufficiently large scale, and having the physical features accurately portrayed. The maps of the Ordnance Survey may, however, soon be expected for Westmoreland; and with the contour lines of the "six-inch" scale, observations on the glacial and drift phenomena (which are so closely connected with relative and actual elevations) will be much facilitated. Mr J. Ruthven's Geological Map, in which the formations are traced according to the classification of Professor Sedgwick, is generally accurate as regards the geological boundaries, but is defective in the shading, a defect which is painfully felt when dealing with the local causes which have guided the movements of old ice-streams.

Northern Drift.—The Carboniferous district of Lancashire, stretching southward from Morecambe Bay, is covered by a thick accumulation of gravel, generally in a matrix of Boulder clay, often forming terraces along the banks of rivers to a depth of 100 feet. This gravel, with boulders of all sizes, is formed of the detritus of the mountainous district immediately northward; and along with blocks of slate, grit, and porphyry, there are included, south of Kendal, boulders of Carboniferous limestone from the hills of that formation, which form the advanced outposts of the mountains towards the south. I mention these blocks particularly for this reason, that they are derived from ranges of hills too low ever to have been the seat of glaciers, so that their presence, so far from their parent masses, must be due either to the transporting power of shore ice, or to their having become imbedded in the bergs of ice which, as shall presently be shown, drifted down from the loughs and fiords of the interior.

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* For fuller descriptions of the physical features and geological structure of the Lake District, see the Memoirs of Professor Sedgwick and Mr W. Hopkins, in the Transactions of the Geological Society, London.

† Dr Buckland, Mr Darwin, and more recently Professor Ramsay, who, in a recent work of the Alpine Club, "Peaks, Passes, and Glaciers," has described in great detail the glacial vestiges of the Snowdon range.

On approaching the valley of Windermere, the depth of the Drift as a whole decreases; and, owing to the irregularity of its bed, it occurs in force only in the hollows and along the flanks of the valleys. As we trace it towards the mountains, it loses its stratified aspect, and becomes a chaotic accumulation of rock fragments, generally rounded or sub-angular, frequently striated and imbedded in a base of mud, covering the flanks of the hills to a height of about 800 feet. At the head of the valley of Windermere it forms somewhat terraced surfaces, distinguishing it from moraines, which in this district almost invariably occur as an aggregation of rounded heaps, differing altogether from any form assumed by marine Drift.

The shape of the moraines, such as that of the valleys of Easdale, Grisedale, and at the head of Langdale, is indeed quite peculiar, resembling a collection of large barrows; and it is a point on which I am still in doubt, whether their present aspect is that which they originally assumed, or whether it is due to atmospheric agencies.

Rock-surfaces.—The hills of Ireleth slate and Coniston grit, which enclose the lakes of Coniston, Esthwaite, and Windermere, in striking contrast to the rugged outline of the mountains of "chloritic slate and porphyry" (Professor Sedgwick) of the interior, have received a rounded and undulating outline. The Coniston flags, however, which form an intermediate zone, from their extreme liability to split along the planes of jointage, cleavage, and bedding, frequently rise in small serried ridges, running parallel to the strike (north-east to south-west).

The cause of this rounded outline may, I think, be clearly traced to the wearing and grinding agency of ice, not in the form of glaciers, but as floating bergs. On close examination, the rock-surfaces are found resolving themselves into rounded and oval bosses, frequently grooved and striated in lines radiating from the interior mountains.

These appearances must be due to the wearing action of floating ice; for, independently of other considerations, it is impossible to suppose that this comparatively low-lying district was overspread by one broad sheet of ice *as a glacier*. This moulded form of rock surface prevails at least as far south as Kendal and the southern extremity of Windermere. The striations and groovings range towards the south, scarcely ever deviating more than 20° from the meridian; but, as remarked by Professor Ramsay in the case of the striated surfaces of North Wales, it is only when the natural covering of detritus or turf has been recently removed that the finer striations become apparent. In general, the oval or rounded forms of the naked rock—even where it stands, and has stood for ages, prominently out of reach of all protection—is faithfully preserved, while the finer ice-marks have yielded to atmospheric erosion acting along the planes of cleavage (see fig. 1). The hollows are generally filled with drift-gravel in a base of red clay, with boulders of porphyry, which sometimes rest as perched blocks on the slates and grits of the Coniston series. As particular points where the striations may be observed, I may mention the Coniston road, above Hawkshead; the porphyritic rocks, both on the east and west flanks of Skelwith Fell; a remarkably fine example on the Coniston road, half a mile south of Brathay Church; and several positions in the " Old Hundred" of Troutbeck, above Ambleside.

Passing now to the consideration of the glacial vestiges of the higher valleys, I shall describe them under the two heads in which they appear naturally to arrange themselves in a glacial point of view.

1*st*, Those valleys which have been channels both for floating ice and glaciers.

2*d*, Those which have been occupied by glaciers only.

The first class comprises the lower and larger valleys of the mountainous district, as Windermere, Coniston, Little and Great Langdale, Ambleside, Grasmere and Troutbeck. They all exemplify glacial phenomena; but, with the exception of their higher reaches, they do not appear to have been occupied by glaciers. For reasons presently to be stated, I attribute these phenomena solely to the action of ice floating down these channels when they were occupied by the glacial sea.

In treating of these valleys I shall commence with that of the Rotha, which was the first to come under my observation, and which forms the principal prolongation of the great valley of Windermere.

Valley of the Rotha (*Ambleside and Grasmere*).—At the head of Windermere, the grooved surfaces—of which I have already given examples—are continued into the valley of the Rotha, and rise on its flanks to a height of about 400 feet above the level of the river, as may be observed along the road to Kirkstone Pass, above Ambleside. All along the alluvial bed of the valley, from Ambleside to a considerable distance beyond Grasmere, the examples of well-formed *roches moutonnées* are both numerous and striking (fig. 1). On one of these ice-worn bosses, fluted and grooved with striations ranging due south, the new church of Ambleside has been erected; and grouped around are several fine examples. One feature in these bosses deserves special notice. When viewed sideways, they appear as prostrate cones or wedges pointing up the valley, or nearly north; thus proving the movement of the ice to have been from north to south. These bosses are at an elevation of 130 feet above the sea.

On the road to Patterdale, at an elevation of about 600 feet, the striations are exactly parallel to those of the bottom of the valley, though *they cross transversely the ridge* which divides Stockdale from Scandale, a proof that the direction of motion was here independent of the form of the valley. Higher up, by the sides of Rydal and Grasmere lakes, the striations are parallel to the longitudinal axis of the valley. The flanks are covered with Drift to an elevation of about 400 feet above Grasmere. In some places this formation presents a slightly terraced surface; and its generally smoothed and undulating slopes, similar to those which the Drift assumes in Lancashire and Cheshire, show that it is a truly marine deposit, and not to be classed with the products of subærial glaciers. It may be traced from Windermere valley into those of Stockdale, Troutbeck, and Scandale, gradually ascending along their sides till it passes into moraine gravel, at an average elevation of 850 to 900 feet.

In composition and want of arrangement this marine Drift resembles moraine matter, of which it is indeed only another form. It is to be recollected, however, that neither the circumstances under which it was deposited, nor the materials themselves, were favourable to the development of planes of bedding. It is composed of gravel and boulders, angular and rounded, polished and striated, imbedded in a matrix of clay of various hues, from dull brown to deep red and purple. This latter colour prevails much in the glacial accumulations of the higher valleys; and I could not but recollect, in connection therewith, the red colour of the boulder-clay of Lancashire. On close examination, it was evident that the clay itself was formed by the trituration and decomposition of the felspathic rocks of the mountains.

Several fine examples of *roches moutonnées* occur above Grasmere—lying as cones, or inclined planes, with their apices pointing up the valley; and when we enter Easdale, and examine the surfaces of the rocks on both sides of the valley along the western flank of Helm Crag, it is impossible not to be struck with the ice-moulded forms which they assume up to an elevation of about 1000 feet.

Great Langdale.—The glaciated aspect of the rocks at the entrance to this valley is conspicuous, especially along the flank of Skelwith Fell. About half a mile from Brathay Church, on the Coniston road, the surface of the "Coniston flags" has been bared for several yards, and exhibits a system of parallel flutings and striations, ranging S.S.E. This, within the limits of a few degrees, is the invariable direction of the striation south of the Brathay, and is independent of the form of the ground. The ice has pursued the same southerly course, whether ascending along the flanks of Skelwith Fell and Oxen Fell, or crossing the more depressed district at the head of Windermere; and it is astonishing how completely the inequalities of the ground seem to have been ignored.

An interesting section in Boulder-clay occurs N.W. of Loughrigg Tarn; it is of a deep red colour, resting on a grooved surface of felspar porphyry. The chloritic slates, which form the flanks of Great Langdale along the northern shore of Elter Water, are remarkably ice-moulded and grooved, to an elevation of 650 feet above the lake. Along the bottom of the valley *roches moutonnées* protrude, and boulders are abundantly strewn over the surface. The striæ range E. 10° S. in the line of the axis of the valley. At the village of Langdale, ice-moulded, polished, and fluted surfaces are remarkably fresh; and I would here remark, that the fine-grained slates of this part of Langdale exhibit the glacial striations in greater perfection than the harder trap-rocks of other parts, the latter having suffered more by weathering along the planes of cleavage. The oval and generally glaciated surfaces are sufficiently evident in the bedded porphyry which sets in above the village, but the fine groovings and striations are seldom exhibited.

Langdale Moraine.—Although the bottom and sides of Langdale present frequent instances of perched and strewn blocks and accumulations of gravel, there does not appear to be any object which might not have been produced by *floating ice*, until we arrive within half a mile of its upper extremity. Indeed, the ascent of this valley is so very gradual that it is improbable glacial ice could have moved along it; and the upper limit of the ice-worn surface seems to preserve a perfect level along the sides of the valley, which may be distinctly traced by the eye, from some positions above Elter Water, at 650 to 700 feet above that lake. Combining these circumstances with the fact that we meet with no undoubted instance of a moraine till we approach the head of the vale, it appears to me the more probable supposition, that the glacial evidences of this valley (as already stated) are attributable to the action of icebergs floating down when it was filled by an arm of the sea, to a level of about 1000 feet.

On approaching the ascent to the Stake Pass, at a height of about 500 feet, we meet with a well-defined moraine, formed of large rounded mounds of gravel, and strewn with boulders. This moraine at one time probably crossed the entire valley, and must have produced a lake, till the mountain torrent, which everywhere has left evidences of its power, hewed for itself the channel which it now occupies. Although at so low an elevation, this moraine to all appearance has never been covered by the sea, and is one of several proofs that the glaciers have occupied the valleys after the sea had retired. (Pl. I., fig. 3.)

The head of the valley is surrounded by a steep though not vertical wall of felspar porphyry and slate, stretching from the northern shoulder of Bowfell to the Langdale Pikes. The Stake Pass crosses the lowest portion of the ridge at an elevation of about 1500 feet, and forms a portion of the central watershed. On reaching the level of the pass, I was surprised to find the whole surface covered by mounds of gravel and boulders, extending some distance down the valley towards Borrowdale, and bounded by the crags of the northern shoulder of Bowfell (see Plate I., fig. 2). It is not improbable that this may be the terminal moraine of a glacier which descended along the higher reaches of the Derwent; but its occurrence in such a position appears to me not easily accounted for. The aspect of this moraine resembles a vast collection of tumuli; and fancy might well point to it as the sepulchre of a battlefield.

Plate I., fig. 3, represents the relative positions of the moraines of Great Langdale and of the Stake Pass.

Little Langdale.—Towards its head, Little Langdale parts into three branches. The north branch leads up to Blea Tarn, and contains fine examples of *roches moutonnées*, perched blocks, boulders, and glacial striations. These last, near the Tarn, point S.E. down the valley.

The central branch contains a well-formed moraine (Plate II. fig. 4), which probably at one time formed an embankment across the valley, in which case it would have produced a lake; but the mountain torrent has scooped a channel between the southern side of the moraine and the mammillated bosses of porphyry which form the ridge.

The southern branch skirts the base of Wetherlam; and, judging by the remarkably smoothed and grooved surfaces of its flanks, has been subjected to an intense degree of glacial action. Around the copper works, the striations are visible at many points, and range nearly due east, in the direction of the valley. This direction is continuous beyond Little Langdale Tarn; but here the boundary-ridge is interrupted by an opening trending southward, and immediately the striations bend round in that direction. The flanks of Oxen Fell and Skelwith Fell also exhibit the southerly striations; and it is evident, from this and similar instances, *that some influence had been constantly acting upon the ice, forcing it to the southward.* Where the ice has held an easterly course, it is due entirely to the elevation of the ridges which bound the valleys. If we suppose these influences to have been either prevalent north winds or currents of the sea, it is an additional evidence that the glaciation of these valleys has been produced by *floating ice,* up to a certain level.

Stockdale.—The flanks of this and the adjoining valleys of Scandale, Rydale, and Troutbeck, are overspread by a heterogeneous accumulation of gravel and boulders in a matrix of red and gray clay, which towards the lower parts of the valleys assumes a somewhat terraced aspect. I have already stated that this is probably *not* true moraine matter, but the incipient form of marine Drift. Towards the head of Stockdale, however, at a height of 950 or 1000 feet, it passes into a true moraine, which occupies the head of Kirkstone Pass (1200 feet), and extends down on the northern side into Patterdale for about 300 yards. On this side the moraine is more perfectly formed than on the side of Stockdale, and consists of the usual assemblage of rounded mounds of gravel, with strewn and perched blocks, covering polished surfaces of cleaved felspar porphyry. I do not think, however, that the remarkable block from which the pass derives its name is a true *glacier*-perched block, but has fallen from the crags which bound the pass.

This is the last of the valleys which I shall attempt to describe on the southern side of the watershed. The description of those on the northern side I propose reserving for a future occasion; and shall content myself with remarking, that the great valleys of that part of the district exhibit similar phenomena, the striations and course of the ice having been northward. Thus the flanks of Grisedale, one of the wildest gorges in the Lake District, having its sources in the heart of Helvellyn, are ice-moulded up to an average elevation of 600 feet or more above the bed of the river. There are also remarkable examples of perched blocks of huge size, and well-defined, if not extensive, lateral moraines. That this valley has been the trough of a glacier, extending almost to its entrance, there is the clearest evidence, in the existence of a large terminal moraine. The elevation above the sea of this moraine is not more than 550 or 600 feet; and its position is within a short distance of the entrance into Patterdale and the head of Ulleswater (380 feet). The moraine is about a quarter of a mile in length, and formerly extended right across the valley; but the impetuous mountain torrent has hewn a channel, and has levelled the ground for a breadth of 100 yards. This glacier, extending from Grisedale Tarn to the terminal moraine, was three miles in length, with an average breadth of 400 yards; and, judging from the height of the polished surfaces along the flanks of the valley, 600 to 800 feet in depth.

If I have hesitated in the cases of the valleys of Langdale, Grasmere, Rydale, Stockdale, and Troutbeck, to admit of the existence of glaciers occupying *their entire lengths*, I certainly have no such hesitation in the case of Grisedale, where the terminal moraine, placed near the very mouth, leaves no room for scepticism. In the case of the former valleys, the well-defined moraines are situated near their heads; and the glacial phenomena which are exhibited along the bottom and sides of these valleys, and extended beyond into the open country, being such as are known to be producable by the action of floating ice, I am disposed to refer them to this agency. Without entering upon the question whether the valleys of the first class may not, at an earlier stage of the glacial epoch, have been occupied by glaciers, I only here maintain, that the marks of the former presence of ice are such as are capable of being produced by bergs floating down from glaciers which precipitated themselves into the sea, as in Tierra del Fuego, and, at a former period, in Scandinavia, where the rock surfaces along the fiords are grooved and ice-worn to and below the water's edge.

The two great facts to which the glacial evidences of the Lake District (as far as I have observed) appear to point are these: first, that the sea stood at a level sufficient to float ice charged with boulders over ridges and hills, which are now at elevations of 800 to 900 feet; and, secondly, that after *the sea had retired,* glaciers descended the valleys as low as 500 feet above the present sea-level.

The reader will therefore bear in mind that I have not now entered upon the question of any changes previous to these, the most recent and most apparent.

Tarns.—The production of tarns, or small mountain-lakes, by the agency either of moraines forming embankments, or by the scooping action of glacier ice, has been illustrated in the case of the High Alps and North Wales by Professor Ramsay.* Not less satisfactory are the examples of this kind in the "Lake District," where these lonely basins of water abound; and distinguishing between *tarns* and the larger and less elevated sheets to which the term "lakes" more properly belong, I think we may safely assert that there is scarcely one of the former in whose production glacial ice has not been concerned. Indeed, as a scientific distinction, it might be advisable to restrict the term "tarn" to those lakes which can be traced to the formation of glacial agency.

Of the tarns belonging to the Southern Watershed, perhaps the most interesting examples, as connected with glacial vestiges, are Stickle Tarn, Easdale Tarn, and Blea Tarn, which I shall describe in this order:—

Stickle Tarn.—This basin is fed by a stream which descends a gorge scooped out along the north-eastern flank of the Langdale Pikes. The sloping ledges of porphyry, which form a conspicuous feature at the upper sources of the brook, are striated E. and W. in the direction of the valley; and at an elevation of about 1800 feet, lateral moraine shingle, formed of deep red gravelly clay, with perched blocks, is strewn along the flanks. The tarn, which reposes at the lower end of this valley, is bounded through half its circumference by a wall of precipitous cliffs, whose extremities are united by a well-defined moraine—thus forming and completing the basin. The interior edge of the moraine is but slightly curved, and forms rather the chord of the arc. It consists, as usual, of large mounds of gravel and clay, on which boulders lie scattered in all positions; and over the south-eastern extremity of the moraine, the brook to which the tarn gives birth leaps forth, and is precipitated in a succession of cascades into Langdale. From the ice-worn character of the rock surfaces along the sides of the tarn, it may easily be inferred that the glacier which deposited the moraine, and thus became the primary agent in the formation of the tarn, at one time cascaded down the declivities.

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* Quar. Jour. Geol. Soc., vol. viii., &c., "Peaks, Passes, and Glaciers," 3d edit.

But in this remarkable little tarn, there is an object almost unique in its kind, and which the tourist frequently notices without being aware of its true meaning. At about the centre of the tarn, a smooth, oval boss of rock, about twelve feet in length, rises about two feet above the water at its centre. On this is perched a natural monument of the transporting agency of ice in the form of a boulder, nearly round, and (judging at a distance of 100 yards) about four feet in diameter (Plate II. fig. 5). It has a most singular effect, thus placed in solitude, and isolated, by a circular sheet of still water, from the blocks, which strew the banks of the tarn in profusion. By its permanence in so critical a position, it shows how completely the lake has been protected from storms, and from changes in the level, as a combination of these circumstances would probably have long since swept the block into the depths of the lake.

Easdale Tarn.—Crossing to the east, over a tract of rocky ground of an elevation of about 2000 feet, we arrive at the edge of the deep basin which incloses Easdale Tarn. That the valley of Easdale was occupied by a glacier, there is undoubted evidence on all hands. A moraine of large dimensions stretches out from the base of the crags above the tarn, and threatens to block the valley, which perhaps it once did. The flanks of the valley are strewn with boulders and perched blocks of huge dimensions; and the dark mammillated masses of trap are conspicuous, especially south of the lower end of the tarn, where they resemble the upturned sides of a ship, grooved with glacial striæ ranging east, visible even at a distance when the beams of the western sun glance athwart their flanks. This smoothed and ice-moulded character of the rocks is apparent to the junction of the valley with that of Grasmere; and is also exhibited along the flanks of Helm Crag, which forms the boundary of the valley on the north-east side. It is probable that the lower end of the glacier entered the head of Grasmere valley by the channel at the foot of Helm Crag, while a portion of the ice lined the summits of the cliffs over which the waters of Sour Milk Ghyll are precipitated, and thus assumed on a smaller scale the appearance of the lower extremity of the "Glacier des Bois."

I have already referred to the moraine at the head of Easdale Tarn. A second and less elevated moraine, partly supported by a ridge of ice-worn trap, strewn with boulders, forms an embankment to the lower end of the lake itself. The brook has worn a channel to a depth of 100 feet into the moraine without reaching the solid rock, so that it is probable that the lake rose higher before the channel was worn to its present depth; and as the process proceeds, it may sink lower.

The perched blocks on the mountain sides along the southern flanks of Easdale are remarkable. They may be seen at elevations of nearly 2000 feet (estimated); and one example, conspicuous at some distance on descending from the tarn towards Sour Milk Ghyll, occupies so critical a position on a shelving ledge, that had the district ever experienced an earthquake, it would infallibly have been precipitated into the valley below.

Blea Tarn.—This little sheet of water occupies a hollow near the pass between the two Langdale valleys. On the east and west the basin is bounded by lofty crags, whose surfaces are glaciated to a height of 250 feet above the tarn. On descending from the tarn towards the main valley, the examples of *roches moutonnées* and perched blocks are numerous and well pronounced. The striations range S. 20° E. A ridge of these ice-moulded bosses of porphyry stretches across the lower or southern side of Blea Tarn, and at first appears to be the agent in banking up the lake; but on following the ridge to the spot where the brook escapes, it will be found that the lake is really due to a small moraine, which fills up a gap between the ridge just mentioned and the rock surfaces at the west side of the tarn. Rock *in situ* does not reach the bed of the brook till it has fallen over several feet vertically of moraine gravel, and boulders.

There are two other little tarns which I visited—those of Grisedale and Loughrigg—whose formation does not appear to be attributable to moraines, as they are completely surrounded by solid rock basins. In the case of Grisedale Tarn, from its situation near the head of a pass or watershed, and partly inclosed by steep and high cliffs, it may not improbably afford an instance of the "*scooping*" power of ice or snow when charged with fragments of rock, to which Professor Ramsay has referred the existence of some of the mountain lakes of Wales.

To recapitulate. The facts here detailed appear to show—

1. That there have been glaciers at different levels, descending as low as 500 or 600 feet after the retirement of the sea which deposited the Drift.

2. That the Drift-sea rose at least as high as 1000 feet, probably much more.

3. That many of the tarns owe their formation to embankments of moraine gravel.

4. *I infer*—that (for reasons stated above) the glaciation of the larger and less elevated valleys, together with the hilly districts south of the mountainous tracts, is due to *floating* ice of the Drift-period, in the form of bergs and coast ice.

5. That there were at this period some agents, whether currents or winds, or both, impelling the icebergs southward, so that they always moved in that direction, unless prevented by barriers.

It is only proper to state in conclusion, that not being furnished with an instrument for measuring altitudes, some of the numbers in the above pages are only estimations. However, as there are many positions—as the summits of the mountains, the more marked prominences and passes, the surfaces of the lakes, &c.—the elevations of which are well known, the author had frequent data to guide him, and feels confident that the numbers stated are close approximations to the reality.

On the Application of certain Laws of Heat and Combustion to the Use and Economy of Fuel. Being the substance of a Paper read before the Aberdeen Philosophical Society, 11th February 1859. By ALEXANDER D. MILNE.

By Combustion is usually meant the chemical combination of two or more bodies with the evolution of light and heat. Our present inquiry refers to combustion effected for the production of heat by means of ordinary fuel.

There are two modes in which this subject may be investigated. We may begin with first principles and ascertained scientific data, and from these descend to general laws and rules for practical guidance; or we may set out by comparing those conditions under which good or bad results have been attained in practice, and thus arrive at general rules which shall be applicable wherever the same conditions or circumstances recur.

The former method we intend chiefly to follow, and shall endeavour to show that many valuable data have been attained by careful scientific research; that they admit of very wide application; and that the deductions are of the highest practical importance to all who employ fuel as a source of heat.

Both the modes of investigation referred to have been ably followed out by Drs Ronalds and Richardson. It is here intended to view the subject in a somewhat different aspect; reducing the more important points to a systematic form, and calling attention to some phases of the inquiry which are not generally understood, although they appear to be of great importance. To illustrate our meaning, we may refer to Table III., in which is shown the inseparable connection between Draught, Initial Temperature, and Economy of Fuel.

The substances generally used as fuel are wood and coal in all their varieties. They owe their heating powers to one or both of two simple substances—to carbon, or to carbon and hydrogen. The heat is evolved as an accompaniment or result of the combination of the oxygen of the atmosphere with the combustible. With carbon, the resulting compound is carbonic acid CO_2, a transparent colourless gas; with hydrogen, the result is water HO, in a state of vapour. The nitrogen of the air passes along with these gases, not altered farther than by being heated to the same temperature as the carbonic acid and vapour of water.

Atmospheric air contains 23 per cent. oxygen and 77 per cent. nitrogen by weight.

The transformation that occurs in the combustion of carbon may be thus represented, atomically:—

				Products of Combustion.	
Carbon,		1 atom =	6·		
Atmos. air, containing	Oxygen,	2 atoms =	16·	Carbonic Acid, =	22·
	Nitrogen,		53·6	Nitrogen,	53·6
			75·6		75·6

Or, assuming carbon as unity,—

			Products.	
Carbon,		1·		
Air, containing	Oxygen,	2·667	Carbonic Acid,	3·667
	Nitrogen,	8·933	Nitrogen,	8·933
		12·6		12·6

In the case of hydrogen, the atom of which is represented by unity, we have,—

			Products.	
Hydrogen,		1·		
Air, containing	Oxygen,	8·	Water,	9·000
	Nitrogen,	26·8	Nitrogen,	26·800
		35·8		35·8

Hydrogen therefore requires, as compared with an equal weight of carbon, three times as much oxygen, and consequently three times as much air.

When we use the term "perfect combustion," we mean that these combinations have occurred.

But combustion may be imperfect. The carbon, or part, may be separated as soot; or if, while the carbon is at an intense heat, the supply of air be suddenly diminished, the oxygen may combine with two atoms carbon instead of one, and thus form carbonic oxide CO, instead of carbonic acid CO_2. Or the hydrogen and carbon, where both are present, may combine, forming hydrocarbons, such as tarry vapours, coal-gas, &c.; or the hydrogen may escape unconsumed. At present, however, we shall consider the laws which are connected with the perfect combustion of carbon and hydrogen separately.

The data relative to the calculations are given in Table I., which explains itself in so far. In columns 5 to 10, the temperature 60° is assumed as the mean; and 32° as that from which it is usual to calculate the expansion of gases by heat.

Columns 13 to 16, relating to specific heat, are of great importance. When we cool different gaseous, liquid, or solid bodies, they give out different quantities of heat; which, expressed numerically, are called specific heats. Two standards are used for comparison—air and water; to the latter we shall adhere in subsequent calculations. If water is assumed as 1·000, air is ·2669; that is, water requires nearly four times as much caloric to raise its temperature any given number of degrees as is required by air.

The specific heats, by different observers, vary to some extent. We give those generally adopted. It yet remains to be determined whether specific heat continues constant at all temperatures: experiments by MM. Dulong and Petit would seem to indicate that at high temperatures, say 1000°, gases require more caloric to raise their temperature than at low temperatures.

The heating powers in column 17 are by Andrews, and are expressed in parts by weight of water heated 1° Fahr. by the caloric generated in the combustion of 1 part by weight of the combustible. The data may be relied on, as the results for carbon by Despretz, Grassi, and Andrews, vary only 2 per cent.; and for hydrogen, the results by Dulong, Hess, Crawford, Grassi, Fabre and Selbermann, and Andrews, vary only 8 per cent.

The results for compound combustibles, such as olefiant gas, containing both carbon and hydrogen, approach closely to the theoretical results as calculated from the separate heating powers of their components. Column 18 shows the approximation in the cases of olefiant and light carburetted hydrogen gases. In most treatises on fuel, the subject is rendered unnecessarily intricate by assigning to each hydrocarbon a separate heating power. They should evidently be regarded as mixtures of so much hydrogen with so much gaseous carbon, because, in their ordinary combustion, they are previously decomposed by the influence of heat into their two elements, which burn in succession; as in the case of illuminating gas.

Carbon as a Source of Heat.

More or less pure, it is met with in various forms, as fuel. We will proceed to give the particulars of its "perfect combustion," in a tabular form, and also examine some of the phenomena which attend its application to heating purposes.

In Table II., the process of combustion, without reference to its application, is represented.

All the caloric generated is regarded as being present in the products of combustion—viz., the carbonic acid and nitrogen. The mean specific heat of this gaseous mixture is found in column 8; in order to ascertain the temperature to which it is raised, as in column 11; and the total amount of sensible heat it contains, as in column 10.

As regards column 10, experimenters having ascertained that the caloric generated by 1 part carbon heats 14,220 parts water (specific heat 1·000) 1° Fahr., it becomes a simple matter to find how many parts of the products of combustion (specific heat ·2596) will be raised 1° Fahr. ·2596 : 1·000 : : 14,220 : 54,776; which may either be regarded as parts heated 1°, or one part heated 54,776°, if such were possible.

To find the temperature, column 11, we have to consider—If the heat generated is such as will raise 1 lb. of the gaseous products 54,776°, what will be the temperature, seeing that the weight of these products is 12·6 lb. Thus,—54,776° ÷ 12·6 = 4347°, which is the intensity of temperature produced, supposing no loss by radiation or conduction, and the specific heats to be the same at high temperatures as at low.

Effect of Excess of Air.—We have seen that 1 lb. carbon in being burned requires 11·6 lb. air, and that the temperature produced is 4300° to 4400°, when exactly this quantity of air is used. But if excess of air pass through the ignited carbon, or enter by any other channel, so as to mingle with the true products of combustion, it will share in the caloric generated. Suppose twice the requisite amount admitted, or 11·6 lb. in excess for 1 lb. carbon, what effect is produced?

In the gaseous current we now have,—

Carbonic acid,	as before,	3·667	lb. at	·2210	specific heat.
Nitrogen,	do.,	8·933	„	·2754	„
Air,	in excess,	11·600	„	·2669	„
	Sum,	24·200	lb. at	·2631	mean sp. heat.
	Instead of,	12·600	lb. at	·2596	do.

We may now find the heating effect of the caloric generated on this new gaseous mixture—and also its temperature.

Heating Effect.—·2631 : 1·0000 : : 14,220° : 54,048°, the temperature to which 1 lb. of the mixed gases would be raised by the heat generated in the combustion of 1 lb. carbon.

Temperature.—54,048° diffused over 24·2 lb. gives 2347°, which is the utmost temperature to which this new mixture can possibly rise; instead of 4347° with the minimum quantity of air.

On this point we may remark generally,—

1. The caloric *generated* is neither increased nor diminished by excess of air. In all cases, it is capable of communicating 14,220° heat to cold water.

2. But this caloric is diffused through a greater quantity of matter when excess of air is used, and the consequence is a reduction of temperature as we increase the excess.

3. The *degree* of reduction may be found as above, by taking into account the increased quantity of gaseous matter in connection with the varying specific heat.

4. In the combustion of carbon, as the specific heat of air is nearly the same as that of the original products of combustion, the mean specific heats, with various degrees of excess, vary very little. The reduction of temperature is therefore almost exactly in proportion to the increase in weight. (This does not hold good in the combustion of hydrogen.)

Application of the Heat from Combustion of Carbon to Praacticl purposes.

To present some features of this part of the subject at one view, Table III. has been drawn up. Its minuteness will obviate the necessity of repeating the calculations, and will also render more evident the mutual dependence of the various principles involved.

By 1 draught, or 1 equivalent of air or draught, we mean that no more air has been admitted than is necessary for perfect combustion—viz., 11·6 lb. per lb. carbon. 2 draught means 11·6 lb. in excess, and so on. We have assumed draught in excess from 1 to 2 equivalents rising by tenths, and from 2 to 20 by greater intervals, and it includes all air mingling with the products of combustion in any way.

Many persons do not seem to be aware of the extent to which excess of draught frequently exists in the ordinary use of fuel, and such may hastily conclude that the principles we advert to, and the formulæ founded on them, are inapplicable. We remark, therefore—

1. It is well known that draught may be so increased that with a thin open stratum of fuel the fire will be at last extinguished.

2. The oxygen of the air must come into immediate contact with every particle of carbon before combination takes place; but it frequently occurs that parts of furnaces, more especially towards the fire-bridge, are not covered with fuel, and other parts so thinly as to allow the air to pass in streams.

3. Air is often admitted by openings above the fuel, with or without intention, at improper times. Such air is not at all required where the fuel is entirely carbonaceous,—such as coke &c.,—as the combustion is entirely confined to the fuel on the grate; and in hydrogenous fuels, it is required only during the period when the inflammable hydrogen compounds are being evolved, and not at all when the carbonaceous residue or cinder is burning alone on the hearth.

4. Many instances occur where the same amount of draught is maintained, whatever be the fluctuations of work done or fuel consumed.

5. The analyses of waste products by various observers show great excess in many cases.

In Table III., for every degree of excess of air, we have given the new mean specific heat (column 6); the heating power constant on cold water (8), but varying slightly on the increased products (9); the pyrometrical effect, or utmost temperature attainable in each case (10); and the diminution of temperature per cent. (11). The most important points here involved are—

1. *Absolute amount of Caloric generated.*—As before remarked, this is the same in all cases where carbon is converted into carbonic acid by combustion in air. The various observers weighed the carbon, but not the air; and their results coincide. It must be remembered that they cooled down the products to their original temperature by ice or cold water.

This heat is capable of communicating 1° Fahr. to 14,220 lb. water per lb. carbon, but its value may be otherwise expressed, according to the purpose to which it is to be applied.

Expressed in pounds water raised from freezing to boiling,—

$$32^\circ \text{ to } 212^\circ = 180^\circ. \quad 14{,}220 \div 180 = 79 \text{ lb.}$$

Raised from mean temperature to boiling point,—

$$60^\circ \text{ to } 212^\circ = 152^\circ. \quad 14{,}220 \div 152 = 93\tfrac{1}{2} \text{ lb.}$$

Raised into steam from mean temperature,—

$$\left.\begin{array}{r}60^\circ \text{ to } 212^\circ = 152^\circ\\ \text{Latent heat in steam, } 965^\circ\end{array}\right\} 1117^\circ. \quad 14{\cdot}220^\circ \div 1117 = 12{\cdot}73 \text{ lb.}$$

Raised into steam from boiling point,—

$$\text{Latent heat in steam, } 965^\circ. \quad 14{,}220 \div 965 = 14{\cdot}74 \text{ lb.}$$

2. *Utmost Temperature, or Pyrometric Intensity.*—In column 10, this is given for every degree of draught. The mode in which it is determined has been already indicated.

This temperature can be expected in practice only when all the heat produced is present in the gaseous current, at the point where we look for such temperature. In most cases part of it is immediately radiated and absorbed, as in steam boilers where the furnace is inside or immediately under; but where the fire is surrounded by non-conducting material, such as brick, the temperature in the space immediately over the fuel may be expected to agree generally with the Table.

An inspection of the Table will show, that with every addition, however small, of air in excess, the utmost temperature attainable decreases. For instance, the current of hot gases rising from a fire of charcoal with a draught of 2 equivalents, or double that requisite for perfect combustion, would indicate about 2000° were a pyrometer freely suspended in it.

By reference to column 6, it will be seen that the mean specific heat of the current varies only slightly as we increase the proportion of air. The diminution of temperature is therefore almost exactly in proportion to the increase of draught, and we are thus able to reduce the matter to a formula or rule easily remembered.

Formula 1. To find the initial temperature due to any amount of air or draught, divide 4400° by the equivalents of air.

The results by this formula coincide closely with column 10.

Example.—Required the initial temperature where 1½ equivalents of air are given, or a draught of $17\frac{4}{10}$ lb. air, per lb. carbon, instead of 11·6 lb.? 4400 ÷ 1·5 = 2933°. With double draught? 4400° ÷ 2 = 2200°. With ten-fold draught? 4400° ÷ 10 = 440°.

By the term "Initial Temperature," as used above and subsequently, we mean the temperature of the gaseous current arising from combustion, after all the atmospheric air from any opening whatever has been added to it, and before any of the caloric has been abstracted.

This formula applies, without any material qualification, to the use of all purely carbonaceous fuels, such as coke, wood, charcoal, anthracite, &c.; and as the laws of nature are inexorable, we must suit our modes of procedure to their requirements. We will afterwards see the important bearing of initial temperature on economy of fuel, extent of heating-surface in boilers, &c.

To the use of common coal or wood in its natural condition, this rule also applies; *with* a qualification, however. Immediately after fuelling, and while the volatile hydrogen and hydrocarbons are being evolved, air ought to be admitted above the fuel, or through the fuel in excess, because these volatile elements burn *in the furnace,* and not *on the hearth.* And while this goes on, the initial temperature due to the carbon will be modified by that due to the hydrogen, which is about one-fifth less for the same proportion of air. But so soon as the hydrogen element disappears, we have carbon alone, in the shape of charcoal, coke, or cinder, and at this stage all fuels become amenable to the law under consideration.

In many applications of fuel, intensity of temperature is required, as in metallurgic operations, the manufacture of alkalies, &c. Cast-iron melts at a temperature of 3000° to 3300°; a draught of 1·5, producing a temperature of 2951°, could not, however long continued, melt a single grain of that metal. 10 draught might boil water, but not melt lead; 5 draught, giving 908° temperature, might melt lead, but not copper.

In all such cases, the quantity of fuel does not decide the temperature, or determine the effect; all depends on the relative amounts of fuel and air. A very small opening in the door or elsewhere of a reverberatory furnace may cause much loss of time and fuel, by its paralyzing effect on the temperature of the gaseous current.

3. *Amount of Heat carried off by the "Waste" Products of the Combustion of Carbon.*—By "waste products," we mean the gaseous current after it has been applied to heating purposes.

As before remarked, carbon generates 14,220° heat; but to obtain it all, we must cool the gaseous current to its original temperature—that of the atmosphere. In the testing experiments by Andrews and others, this was done, but is not practicable in the ordinary use of fuel. The departing current must of necessity be, in all cases, as warm as, and is in general much warmer than, the surface or body to which it has communicated caloric. This involves loss of heat; and we will now consider the laws which regulate the loss under varying circumstances, and draw such general conclusions as may be practically useful.

Suppose a current of heated air to issue from an orifice; what data are required in order to estimate the amount of caloric contained in it, as compared with the amount in the same current at atmospheric temperature? The data are—1. Its weight; 2. Its specific heat; 3. Its temperature.

Example.—In one hour 1000 lb. gaseous matter—specific heat ·2596, temperature 560°—escape. Required the caloric present over that present at a temperature of 60°.

1000 lb. at ·2596 sp. heat = 259·6 lb. water at 1·0000 sp. heat, and
259·6 lb. water heated 500° = 129,800 lb. water, heated 1°,

which would be the amount of caloric lost in one hour.

Suppose, also, that during the same period we had consumed 120 lb. carbon, what is the loss per cent.?

Total heat generated, 14,220° × 120 = 1,706,400°.
Loss as above, 129,800° = 7·6 per cent.

In general we may therefore affirm,—

1. The loss by heated waste products increases with the temperature at the moment of escape from contact with the body receiving the caloric. In future, it will be convenient to call this the "Terminal Temperature."

2. The loss also increases with the quantity or weight of the waste products.

3. Also with the specific heat. But, as before observed (column 6, Table III.), this element does not vary to any notable extent in burning carbon with varying equivalents of air. Under all circumstances it ranges from ·2596 to ·2669.

But as the subject admits of more rigid calculation, and as we believe the principles involved to be of the highest importance in practice, we have inserted the columns in Table III., immediately succeeding column 14. This latter column contains, for each case, the weight of water, equal in capacity for heat to the whole gaseous products per lb. carbon. For example, the heat which would raise the waste products from 1 lb. carbon, with a draught of 1½, 200° temperature, as indicated by a thermometer, would raise 4·82 lb. water 200°, or 1 lb. water 964° (= 4·82 × 200°), as in the Table. By multiplying column 14 by the degrees of temperature in the escaping gases, we thus obtain the loss for any terminal temperature *in degrees of heat on 1 lb. water per lb. carbon consumed.* Now, each lb. carbon generates 14,220°; so that we are able to find the loss of heat per cent. in each case.

Each result given in the Table is calculated in the same manner.

Terminal temperatures up to 550° may be ascertained by means of a mercurial thermometer. For higher temperatures other means must be employed. A reliable pyrometer for high temperatures is still a desideratum. In most steam-boilers working stationary engines, where economy is at all consulted, the mercurial thermometer ought to be sufficient.

Although the calculations have rather a formidable appearance in detail, the results may be got at once by a very simple formula.

Formula 2.—To find the per-centage of loss of heat by waste products, multiply the terminal temperature by the equivalents of draught. The constant number 4400 is to the product, as the whole caloric, capable of being generated by the carbon, is to the loss. Examples:—

Terminaltemp. 500° × Draught 1·3 = 650° = 14·8 per cent. less on 4400°.
Do. 300° × do. 1·8 = 540° = 12·2 do.
Do. 600° × do. 2·0 = 1200° = 27·3 do.
Do. 3000° × do. 1·0 = 3000° = 68·2 do.
Do. 4400° × do. 1·0 = 4400° = 100· do.

These results agree closely with those given in the Table; and if we are correct in our premises so far as known scientific data extend, the principle is of immense importance, and deserves attention. The *quantity* of carbon consumed, either as coke, charcoal, cinder, &c., does not affect the result. With a terminal temperature of 600° (which is common), and a draught of 2, or twice the air necessary for perfect combustion (which is equally so), the loss cannot but be, at least, 26·7 per cent. of the heat generated, whether from 100 grains or 100 tons of the carbonaceous material.

When we bear in mind that anthracite and several other varieties of coal contain 90 to 98 per cent. carbon; that charcoal contains about as much, the volatile ingredients having been expelled; and that even ordinary bituminous coal contains 80 to 90 per cent., about 50 per cent. remaining on the hearth as purely carbonaceous coke or cinder, the extensive application of the formula now given must be very evident.

By its aid, many problems which arise in ordinary practice may be solved—at least approximately, or in proportion as we are able to ascertain, with accuracy, the terminal temperature and draught. We give a few examples.

Example.—With one draught, we find the terminal temperature from a boiler-furnace to be 1000°. What saving of fuel might be expected by increasing the boiler surface so as to reduce the terminal temperature to 400°?

1000° × 1 = 1000 = 22·7 per cent loss.
reduced to 400° × 1 = 400 = 9·1 do.

Probable saving, 13·6 per cent.

Example.—With 2 draught, which is much more common than 1, we would have,—

1000° × 2 = 2000 = 45·4 per cent. loss.
reduced to 400° × 2 = 800 = 18·2 do.

Probable saving, 27·2 per cent.

The further effect of properly regulating the draught would be,—

1000° × 2 = 2000 = 45·4 per cent. loss.
reduced to 400° × 1 = 400 = 9·1 do.

Probable saving, 36·3 per cent.

Example.—With 2 draught, and terminal temperature 600°, what saving might we expect from adopting Green's Fuel Economizer, or any similar apparatus, to absorb and utilize the waste heat by reducing the temperature to 200°?

600° × 2 = 1200 = 27·0 per cent. loss.
reduced to 200° × 2 = 400 = 9·1 do.

Probable saving, 17·9 per cent.

Example.—With a draught of $1\frac{3}{10}$, the terminal temperature of a reverberatory furnace is found to be 1500°. Required the saving, if the heat were applied to evaporating purposes, and the temperature reduced to 300°?

$1500° \times 1\tfrac{2}{10} = 1800 = 40{\cdot}9$ per cent. loss.
$300° \times 1\tfrac{2}{10} = 360 = 8{\cdot}2$ do.

Probable saving, 32·7 per cent.

(It may perhaps tend to strengthen confidence in this formula to state that, in the writer's experience, 30 to 35 per cent. saving of fuel has been in some cases effected, by simply reducing and regulating draught, without any reduction of the terminal temperature.)

4. *Excess of Air as diminishing the effective and increasing the non-effective Caloric in heated Gaseous Currents.*—Against diminishing excessive draught, it may be, perhaps, urged, that it will, by raising the initial temperature, also raise the terminal temperature, and so leave us where we were as to economy of fuel. But we shall endeavour to show, that it is *absolutely* impossible to obtain so much heat from the same fuel in the one case as in the other.

Suppose the case of a steam-boiler, steam at 14 lb. pressure. The temperature of the steam and also of the water will be about the same at all points—viz., about 250°. The iron of which the boiler is formed, being the medium through which the heat is communicated, will be somewhat higher in temperature—suppose it 300°. Lastly, suppose this boiler heated by currents of gaseous matter, containing the caloric generated by the combustion of a constant weight of carbon, with varying proportions of air or draught.

It is evident that, under all circumstances, the gaseous current, in the case supposed, must retain at least 300° temperature, that being the temperature of the receiving surface of iron. Up to 300°, therefore, the caloric in the current is *non-effective;* above 300° it is *efficient,* and capable of being communicated, if the heating or absorbing surface is sufficiently extensive.

Referring again to Table III., we find that the waste products, at a terminal temperature of 300°, contain, for varying draughts, the following per-centages of the whole heat generated, which per-centages are, in this case, *non-effective.* 1 lb. carbon generates heat capable of evaporating 14·75 lb. water.

Fuel used.	Heat generated.	Temperature of absorbing surface.	Varying equivalents of draught.	Proportion of effective heat capable of being absorbed.	Proportion of non-effective heat incapable of being absorbed.	Duty of carbon per lb. attainable in lbs. water evaporated.
Constant. 100	Constant. 100	Constant. 300°	1·	93·1	6·9	13·71
100	100	300°	1·5	89·8	10·2	13·24
100	100	300°	2·	86·6	13·4	12·77
100	100	300°	2·5	83·3	16·7	12·08
100	100	300°	3·	80·0	20·0	11·80
100	100	300°	4·	73·5	26·5	10·84
100	100	300°	5·	67·0	33·0	9·88
100	100	300°	10·	34·3	65·7	5·06

The proportion of non-effective to effective rises rapidly, and the loss is absolute and inevitable with these draughts; as the boiler, however extensive its surface, cannot absorb caloric from a current which has fallen to its own temperature. The last column shows the diminishing effect as to the quantity of water raised into steam. We have reason to believe that 5 to 10 equivalents of air are admitted, very frequently, in one way or another; and, if so, the waste of fuel, or rather of heat, *cannot be less* than we have stated; it may be more from the action of other causes.

For practical guidance, we will express the principle involved, in a formula.

Formula 3.—To find the proportion of caloric which is *non-effective* in the use of carbonaceous fuel, multiply the temperature of the body or surface to which the heat is applied by the equivalents of draught. 4400 is to the product as the whole caloric capable of being generated is to the proportion that is non-effective.

This formula differs from the last in one respect only: it gives the minimum loss, supposing our arrangements are most complete, and that all the heat is absorbed which can possibly be; while formula 2 gives the actual loss as determined from the temperature at which the products actually escape, which is generally much higher than that of the absorbing surface or body.

Consideration of the points we have referred to will serve to give definite views as to the economizing of fuel and heat by applying waste products to heating water and other purposes. To render effective a gaseous current which has become non-effective, we have simply to present a body of lower temperature; by this means a farther quantity of caloric is absorbed. We reach the limit when the current becomes so cold as to be incapable of creating sufficient draught by its ascent in the chimney. But however low we may reduce the terminal temperature, excess of draught still continues to increase the proportion of caloric which is finally non-effective.

5. *Excess of Air as it affects the Amount of Surface required for the Absorption of Heat.*—We shall now show that excess of air not only diminishes the effective heating power of gaseous currents, but renders a more extensive surface necessary for the absorption of the same amount of caloric. Or, taking, for example, the raising of steam, we find that as we approach the minimum of draught necessary for perfect combustion, the available heat is not only increased in quantity from the same fuel, but less boiler surface is required than with excess of air.

In general terms, and without anything approaching to demonstration, this ought to be allowed, when we consider that, as the excessive draught is diminished, the temperature is increased; and that as the temperature of a heating body increases, its power of heating another body increases also; and, lastly, that as the heat *received* by the boiler-plate increases, the steam raised from its surface increases in exactly the same ratio.

The gaseous current, as it passes along the flues and sides of a boiler, communicates heat by actual contact; above or around the fire or flame, heat is communicated by radiation also. With regard to the rate or velocity at which a current communicates its heat by contact alone, experiments by Dulong and Petit may be referred to. They found that gases cooled other bodies by contact in the following ratio:—"When the difference of temperature (between the gas and the body) is doubled, the velocity of cooling is increased 2·35-fold." (*Gmelin*, p. 233, vol. i.) The inference is, that *heating* by contact of gases observes the same progression, as a similar agreement exists between the phenomena of radiation and absorption.

But, in the absence of direct experiment as to the applicability of Dulong and Petit's law to *heating* by gases, we shall merely assume that the amount of heat communicated increases with the difference of temperature in arithmetical progression; that is, when we double the difference between the temperature of the current and that of the boiler, double the quantity of caloric is communicated; and so on for other differences. This makes ample allowance for the probability that the part of the boiler surface in contact with the current may increase in temperature to some extent also, as we raise that of the current itself.

Suppose, as before, that, to raise steam, we employ carbonaceous fuel under different degrees of draught, assuming the temperature of the water as 250°, and that of the boiler 300°; carbon used, same weight in every case.

Carbonaceous fuel used,	1	1	1	1	1	1
Varying degrees of draught or air,	1·	1·5	2·	2·5	3·	4·
Corresponding initial temperature in each case, see Table III.,	4347°	2951°	2233°	1797°	1503°	1132°
Temperature of absorbing surface in each case,	300°	300°	300°	300°	300°	300°
Difference of temperatures in each case,	4047°	2651°	1933°	1497°	1203°	832°
Corresponding decrease in the amount of caloric absorbed by an equal amount of boiler surface, taking 1 draught as unity,	*1·00	·65	·48	·37	·30	·20
Proportion of caloric which is effective, or capable of being absorbed in each case, as found by Formula 3,	·93	·90	·86	·83	·80	·73

We thus see that with excess of air above what is chemically required, there is not only a diminution of the amount of heat which can possibly be absorbed (see formula 3), but the absorbing power of the boiler is also diminished, and that in a much higher ratio. We have not only less available caloric, but we require additional absorbing surface to obtain even this smaller amount. The rates at which the surface absorbs caloric or generates steam, as given above (*), refer to that part of the boiler to which the gaseous current is first applied; but its course throughout is influenced in the same way. The first square foot of surface with 1 draught is equal in power of raising steam to the first 5 square feet with 4 of draught; consequently, less remains for the other parts to absorb in the former case. With limited draught, in short, the same boiler surface will absorb much more heat, or a much smaller boiler surface will absorb the same heat. The advantage may be gained, therefore, either in the shape of economy of fuel, or economy of boiler surface.

To construct a formula which shall express accurately the operation of the law here involved is impracticable until farther experiments have been made. The data required are—*first,* The exact ratio which exists between the rate of communication of caloric from gases to absorbing surfaces, and the difference of temperature between the two. *Secondly,* The coefficient of absorption for any given thickness of boiler-plate: for example, how many pounds water will be raised into steam per hour from 1 square foot of boiler-plate ⅜ inch in thickness, exposed to a current whose constant heat is 2000°? Were these data known, it would not be difficult to find mathematical expression for the absorption of caloric at every part of the course of the gaseous current.

Keeping in view that we have hitherto referred to purely carbonaceous fuel, or to that part of ordinary fuel which is carbonaceous, the importance of regulated draught must be evident to all who have followed us, whether previously familiar or not with the principles which regulate combustion.

The endeavour we have made to arrive at precise numerical results will, we feel confident, be appreciated by those who are interested in this important subject. The necessity for such precision will be seen when, on a subsequent occasion, we proceed to apply to practical purposes the principles here laid down.

TABLE I.

Constitution and Physical Properties of the Simple and Compound Bodies chiefly concerned in Ordinary Combustion.

NAME.	Symbol and atomic weight.		Specific gravity of air =1·0000.	Weight of 100 cubic inches at		Weight of 1 cubic foot at		Bulk of 1 lb. at		Constituent parts.		Specific heat.		Hence the caloric which gives 100° temp.		Lb. water raised 1° temperature by combustion of 1 lb.	
				32° Temp.	60° Temp.	32° Temp.	60° Temp.	32° Temp.	60° Temp.	100 parts by weight consist of	100 parts by volume consist of	Water = 1·0000.	Air = 1·0000.	in Water gives	in Air gives	Actual results, by Andrews.	Corrected to agree with carbon and hydrogen.
				grs.	grs.	lb.	lb.	cub. ft.	cub. ft.					°	°		
Oxygen, ...	O	8	1·1093	36·353	34·388	·08974	·08489	11·142	11·780	O 100	O 100	·2361	·8848	423·5	113·0		
Nitrogen, ...	N	14	·9706	31·808	30·089	·07852	·07428	12·735	13·464	N 100	N 100	·2754	1·0318	363·1	96·9		
Hydrogen, ...	H	1	·0693	2·271	2·148	·00561	·00530	178·371	188·571	H 100	H 100	3·2936	12·3401	30·4	8·1	60,840	60,840
Atmospheric Air, ...	...	...	1·0000	32·771	31·000	·08090	·07653	12·361	13·068	O 23, N 77	O 20·75, N 79·25	·2669	1·0000	374·7	100·0		
Carbonic Oxide, ...	CO	14	·9709	31·818	30·098	·07855	·07430	12·731	13·460	O 57·14, C 42·86	O 50, C 100	·2884	1·0805	346·8	92·5		
Carbonic Acid, ...	CO^2	22	1·5252	49·983	47·281	·12339	·11672	8·105	8·568	O 72·73, C 27·27	O 100, C 100	·2210	·8280	120·8	452·4		
Light Carburetted Hydrogen, .	CH^2	8	·5546	18·175	17·193	·04487	·04244	22·288	23·563	H 25, C 75	H 200, C 100	Doubtful.				23,594	25,875
Olefiant, ...	$C^2 H^2$	14	·9706	31·808	30·089	·07852	·07428	12·735	13·464	H 14·29, C 85·71	H 200, C 200	·4207	1·5763	237·7	63·5	21,495	20,880
Steam or Vapour of Water, . .	HO	9	·6240	20·449	19·344	·05048	·04775	19·809	20·942	O 89·89, H 11·11	O 100, H 200	·8470	3·1360	118·0	31·9		
Water in liquid state, . . .	HO	9	...							O 89·89, H 11·11		1·0000	3·7467	100·0	26·7		
Carbon in combined gaseous state,	C	6	·4159	As computed from sp. grav. of its compounds.						C 100	C 100	Unknown.				14,220	14,220
Carbon in solid state, . . .	C	6								C 100	C 100	Various.				14,220	14,220
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)

TABLE II.

Combustion of Carbon and Hydrogen; Agents and Products; Heating Power and Pyrometrical Intensity.

NAME.	Symbol and atomic weight.		Parts of air, and its components required for combustion of one part in atomic proportions.	Weight of air, and its components required for combustion of 1 lb.	Products of combustion of 1 lb.			Heat generated by the combustion of 1 lb.		Pyrometrical effect with 1 equivalent of air.	Cubic feet of air required per lb.	Volume of air required per volume of combustible gas.	Volume of products per volume gas.
					Weight.	Separate specific heats.	Mean specific heat.	Weight of cold water raised 1° temperature.	Heating effect on the products themselves in lb. raised 1°.	Temp. of the products.			
				lbs.	lbs.			lbs.			cub. ft.	Volume.	Volume.
Hydrogen,	H	1	Oxygen, . 8· Nitrogen, . 26·8 = Air, . . 34·8	Oxygen, . 8· Nitrogen, . 26·8 = Air, . . 34·8	Vapour of Water, 9· Nitrogen, . 26·8 = Products, 35·8	·8470 ·2754	·4191	60,840 Deduct in pyrometrical calculations, and for all heating purposes above 212°, the latent heat in 9 lbs. vapour of water, . 8,685 52,155	124,445	3476°	454 76/112	2·412	2·910
Carbon, .	C	6	Oxygen, . 16· Nitrogen, . 53·6 = Air, . . 69·6	Oxygen, . 2·667 Nitrogen, . 8·933 = Air, . 11·600	Carb. Acid, 3·667 Nitrogen, . 8·933 = Products, 12·600	·2210 ·2754	·2596	14,220	54,776	4347°	151 32/100		
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)			

TABLE III.—*To demonstrate the Mutual Connection between Draught, or Supply of Air,*

Combustion of carbon with various proportions of common air.				Calculations to find the varying specific heat of the waste gases, supposing excess of air used in combustion or allowed to mix with the hot gases in any way.		Heat evolved is such as will raise the temperature 1° Fahr. of		Pyrometrical effect.	Decreasing pyrometrical effect.	Pyrometrical Indications.		Weight and specific heat of the gaseous products or waste gases.		Specific equivalent in lb. water.
In equivalents.		By weight.												
Fuel.	Draught.	Carbon.	Air.			Cold water.	Products of combustion.	Or initial temperature of gaseous current.	Maximum effect = 100.	Appearance of a body such as carbon or soot freely suspended in an atmosphere of varying temperature.	Physical effects of such temperature.	lbs.	Specific heat.	Of specific heat 1·000.
		lbs.	lbs.	lbs.	Sp. ht.	lbs.	lbs.	Fahr.		Pouillet.				
				CO^2 3·667 N 8·933	·2210 ·2754									
1	1	1	11·6	12·60 Air 1·16	·2596 ·2669	14,220	54,776	4347°	100			12·6	·2596	3·27
1	$1\frac{1}{10}$	1	12·76	13·76 1·16	·2602 ·2669	...	54,650	3972°	91			13·76	·2602	3·58
1	$1\frac{2}{10}$	1	13·92	14·92 1·16	·2607 ·2669	...	54,545	3656°	84			14·92	·2607	3·89
1	$1\frac{3}{10}$	1	15·08	16·08 1·16	·2611½ ·2669	...	54,451	3386°	78	Highest heat of wind furnace (Daniell)		16·08	·2611	4·20
1	$1\frac{4}{10}$	1	16·24	17·24 1·16	·2615½ ·2669	...	54,368	3153°	72½			17·24	·2615½	4·51
1	$1\frac{5}{10}$	1	17·40	18·40 1·16	·2619 ·2669	...	54,295	2951°	68	Dazzling white		18·40	·2619	4·82
1	$1\frac{6}{10}$	1	18·56	19·56 1·16	·2622 ·2669	...	54,233	2772°	64		Cast-iron melts	19·56	·2622	5·13
1	$1\frac{7}{10}$	1	19·72	20·72 1·16	·2624½ ·2669	...	54,181	2615°	60	Strong white heat		20·72	·2624½	5·44
1	$1\frac{8}{10}$	1	20·88	21·88 1·16	·2627 ·2669	...	54,130	2474°	57			21·88	·2627	5·75
1	$1\frac{9}{10}$	1	22·04	23·04 1·16	·2629 ·2669	...	54,089	2347°	54	White heat		23·04	·2629	6·05
1	2	1	23·20	24·2 5·8	·2631 ·2669	...	54,048	2233°	51	Bright ignition	Silver melts	24·20	·2631	6·36
1	$2\frac{5}{10}$	1	29·0	30·0 5·8	·2638 ·2669	...	53,904	1797°	41	Full cherry red Bright do.		30·0	·2638	7·91
1	3	1	34·8	35·8 11·6	·2643 ·2669	...	53,802	1503°	35	Commencing cherry red		35·8	·2643	9·46
1	4	1	46·4	47·4 11·6	·2649 ·2669	...	53,680	1132°	26	Dull red		47·4	·2649	12·59
1	5	1	58·0	59·0 11·6	·2653½ ·2669	...	53,589	908°	21	Incipient redness		59·	·2653½	15·65
1	6	1	69·6	70·6 11·6	·2656 ·2669	...	53,539	758°	17½	Black		70·6	·2656	18·75
1	7	1	81·2	82·2 11·6	·2658 ·2669	...	53,499	651°	15	Do.	Mercury boils	82·2	·2658	21·84
1	8	1	92·8	93·8 11·6	·2659 ·2669	...	53,479	570°	13	Do.	Lead melts	93·8	·2659	24·94
1	9	1	104·4	105·4 11·6	·2660 ·2669	...	53,459	507°	12	Do.		105·4	·2660	28·04
1	10	1	116·	117·0 58·0	·2661 ·2669	...	53,439	456°	10½	Do.	Tin melts	117·	·2661	31·13
1	15	1	174·	175·0 58·0	·2664 ·2669	...	53,379	307°	7	Do.		175·	·2664	46·61
1	20	1	232·	233·0	·2665	...	53,359	228°	5	Do.		233·	·2665	62·09
(1)	(2)	(3)	(4)	(5)	(6)	(8)	(9)	(10)	(11)	(A)	(B)	(12)	(13)	(14)

and Intensity of Temperature, Economy of Heat, &c., in the Use of Carbonaceous Fuel.

Column 14, multiplied by the excess of temperature over that of the atmosphere at which the waste gases escape to the chimney, gives the loss of heat in lbs. water heated 1°, for every lb. of carbon used: which loss increases with the terminal temperature, and with the draught as given in columns 2 and 4.

200° Term. temp.		300° Term. temp.		400° Term. temp.		500° Term. temp.		600° Term. temp.		1000° Term. temp.		1500° Term. temp.		2000° Term. temp.		3000° Term. temp.		4000° Term. temp.	
Loss in lbs. water heated 1°.	Loss per cent. on 14,220.	Loss in lbs. water heated 1°.	Loss per cent. on 14,220.	Loss in lbs. water heated 1°.	Loss per cent. on 14,220.	Loss in lbs. water heated 1°.	Loss per cent. on 14,220.	Loss in lbs. water heated 1°.	Loss per cent. on 14,220.	Loss in lbs. water heated 1°.	Loss per cent. on 14,220.	Loss in lbs. water heated 1°.	Loss per cent. on 14,220.	Loss in lbs. water heated 1°.	Loss per cent. on 14,220.	Loss in lbs. water heated 1°.	Loss per cent. on 14,220.	Loss in lbs. water heated 1°.	Loss per cent. on 14,220.
654	4·6	981	6·9	1,308	9·2	1,635	11·5	1,962	13·08	3,270	23·0	4,905	34·5	6,540	46·0	9,810	69·0	13,080	92·0
716	5·0	1,074	7·5	1,432	10·	1,790	12·6	2,148	15·0	3,580	25·2	5,370	37·8	7,160	50·4	10,740	75·5		
778	5·5	1,167	8·2	1,556	11·	1,945	13·7	2,334	16·5	3,890	27·4	5,835	41·0	7,780	54·7	11,670	82·0		
840	5·9	1,260	8·8	1,680	11·8	2,100	14·8	2,520	17·7	4,200	29·5	6,300	44·3	8,400	59·1	12,600	88·6		
902	6·3	1,353	9·5	1,804	12·6	2,250	15·8	2,706	18·9	4,510	31·7	6,765	47·6	9,020	63·4	13,530	95·2		
964	6·8	1,446	10·2	1,928	13·6	2,410	16·9	2,892	20·3	4,820	33·9	7,230	50·9	9,640	67·8				
1,026	7·2	1,539	10·8	2,052	14·4	2,565	18·0	3,078	21·6	5,130	36·0	7,695	54·1	10,260	72·1				
1,088	7·6	1,632	11·4	2,176	15·2	2,720	19·1	3·264	22·8	5,440	38·2	8,160	57·4	10,880	76·5				
1,150	8·1	1,725	12·1	2,300	16·2	2,875	20·2	3,450	24·3	5,750	40·5	8,625	60·7	11,500	80·9				
1,210	8·5	1,815	12·7	2,420	17·0	3,025	21·3	3,630	25·5	6,050	42·5	9,075	63·8	12,100	85·1				
1,272	8·9	1,908	13·4	2,544	17·8	3,180	22·3	3,816	26·7	6,360	44·7	9,540	67·1	12,720	89·4				
1,582	11·1	2,373	16·7	3,164	22·2	3,955	27·8	4,746	33·3	7,910	55·6	11,865	83·4						
1,892	13·3	2,838	20·	3,784	26·6	4,730	33·3	5,676	39·9	9,460	66·5	14,190	99·8						
2,518	17·7	3,777	26·5	5,036	35·4	6·295	44·2	7,554	53·1	12,590	88·5								
3,130	22·0	4,695	33·	6,260	44·0	7,825	55·0	9,390	66·0										
3,750	26·3	5,625	39·5	7,500	52·7	9,375	65·9	11,250	79·1										
4,368	30·7	6,552	46·1	8,736	61·4	10,920	76·8	13,104	92·1										
4,988	35·2	7,482	52·6	9,976	70·1	12,470	87·7												
5,608	39·5	8,412	59·2	11,216	78·9	14,020	98·6												
6,226	43·8	9,339	65·7	12,452	87·6														
9,322	65·6	13,983	98·3																
12,418	87·3																		

On the Disguises of Nature: being an Inquiry into the Laws which regulate External Form and Colour in Plants and Animals. By ANDREW MURRAY, F.R.S.E.*

I have long thought that an inquiry into the laws by which the external—or what may be called the extrinsic—forms and appearance of organic objects are regulated, might worthily and profitably exercise the faculties of some of our deeper thinkers. It is a subject to which, so far as I know, nothing has been done; at all events, no one has yet offered any explanation of what these laws are; and we may even be said to be ignorant whether there be any such laws or not.

In the hope that a statement of the subject may induce some one better qualified than myself to turn his attention to it, I propose to group together a few facts illustrative of some of the different aspects in which the subject may be regarded, and to indicate the interesting nature of the speculations to which they may give rise.

One very curious branch of the subject, with which I shall begin, is the resemblance which certain plants and animals bear to other objects, animate or inanimate, as, for instance, an insect to a leaf, a moth to a bee, &c. I have styled this the disguises of nature; a phrase which, although it does not apply to all the kinds of resemblance which I shall have to notice, sufficiently indicates their general nature. Other resemblances, such as the general family resemblance in the facies of animals spread over a whole continent, or the resemblance to be found in species confined to a particular kind of locality, will also receive a portion of our consideration; but these cannot be called disguises, and will fall to be discussed under a separate category.

The disguise is usually one of two kinds. It is either an imitation of an animate or of an inanimate object. These we may take up separately. Not that I wish it to be thereby inferred that they differ from each other either in their conditions or the laws which regulate them; that is a question upon which we are not yet prepared to adjudicate; but I merely so take them for the convenience of obtaining some kind of order or arrangement into which to group the heterogeneous mass of facts which bear upon the subject.

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I shall first cast a hasty glance over a few of those curious resemblances where we find one animal assuming the form and appearance of another, as a butterfly pretending to be a wasp, a fly a bee, &c.

I do not mean to include in this category those resemblances which the fancy of man loves to discover in all around him. Like Hamlet, we can see a camel or a weasel figured in every cloud, and, like his, they prove often very like a whale. It will be sufficient, as illustrations of such resemblances, to refer to the death's-head moth, which carries on its thorax not a bad figure of a death's head and cross bones; or to the Orchids and other Epiphytes, which we may compare to gaudy butterflies or other insects; or to the fern called *Aspidium Barometz*, known as the Tartarian or vegetable lamb. It grows on the elevated salt plains west of the Volga, and its rhizome presents, when the fronds are removed, a rude resemblance to the shape of the lamb. It is covered by a soft downy substance, of a reddish-brown colour, which may be compared to a fleece. Like the stems of other ferns, the inner parts are soft and pulpy, and the sap of a rich red colour resembling blood. From these materials a number of fabulous stories have been concocted, which are related by Struys and other authors—such as that the plant has the shape and appearance of a lamb, with feet, head, and tail, distinctly formed; that it feeds upon grass, turning round upon its stem to reach it; that garments are made of its fleece; that the wolves feed upon it, and are very fond of its flesh, from the resemblance it bears in taste to the animal lamb; and, in conclusion, after telling a number of such tales, Struys adds with commendable, though somewhat tardy caution, "Many other things I was likewise told, which might however appear scarcely probable to such as have not seen them."

But leaving such fancies, we shall find plenty of curious imitations among plants and animals, so exact, that man's fancy is not required to originate them, but his judgment to eliminate the deception. As we shall presently find in the other class of disguises, so here the greatest number is to be found in the insect tribes.

The clear-wing moths have so great a resemblance to bees, or wasps, or flies, as to have received the name of *Apiforme, Bombiforme, Vespiforme, Tipuliforme,* according as they wear the dress of a honey-bee, a humble-bee, a wasp, or a gnat. Moths and butterflies, although belonging to very distinct divisions, sometimes assume the appearance of each other, as, for instance, *Callimorpha helcita,* and *Danäe Hegesippe,* which are so metamorphosed, that the moth might be taken for the butterfly, or the butterfly for the moth. Many moths also greatly resemble the Caddice flies (*Trichoptera*), as *Adela frischella* and *Molanna angustata.* A moth named *Adactylus Bennettii,* looks very like a small species of *Tipula.* Many two-winged flies have a most striking resemblance to bees, of which I may select, as examples, the *Volucellæ* generally, the *Gasterophilus equi,* or perfect insect of the Bot, and the *Bombylii,* more specially the *Bombylius major,* which carries its disguise farther than usual. Every one knows that the bee has a long tongue; the fly has not; but as if to imitate the long tongue of the bee, which is often extruded, the *Bombylius* has a long rostrum or snout sticking out like the tongue of the bee—the relative proportion being well preserved. The *Asaphes,* although bees, are yet bees of peculiar structure and habits: they make no hives or nests for themselves, but enter the nests of other bees, and deposit their eggs there, to be reared at the expense of the owners of the nest. They are dressed exactly like other species of *Bombi,* and doubtless thus escape detection on their pilfering and illegal expeditions. *Psychoda phalænoides* is a small two-winged fly, which anybody at first sight would mistake for a moth. *Gibbium Scotias* is a beetle, having considerable resemblance to a flea when seen in profile, and a still greater resemblance, when looked at from above, to a small brown spider, which every one sees too much of in summer. Many Coleopterous insects are found in ants' nests, seemingly created to pass their lives there; for some are eyeless, and many so exactly resemble the ants among which they live as to be not easily recognisable. Many examples of similarity between unlike and distant families occur also in this class, although the resemblance chiefly applies to distribution of colour. Take, for instance, some of the *Lycidæ*, and compare them with *Paristemia* and *Pœciloderma*, two genera of Longicorns, where the colour, although most unusual and startling, is distributed exactly in the same manner, and produces the same effect.

In plants, similar resemblances between distinct orders and families exist. I may refer to the tree-fern (*Alsophila*) and the Cycad (*Stangeria*), like each other, and yet belonging to distinct orders; or to the *Sesleria cærulea* and *Carex rupestris;* to the *Potentilla alpestris* and *Ranunculus alpestris;* to *Polygonum Convolvulus* and *Convolvulus sepium;* to *Tamarix gallica*, and many of the cypresses, or to *Calluna vulgaris* and *Hudsonia ericoides*,—all as showing respectively much similarity to each other in the foliage, and yet belonging to different families.

Such are some of the resemblances in the organic creation, which might without impropriety be termed impostures or personations. It is difficult to suggest any probable theory by which they may be accounted for. Some, no doubt, can be explained as being found in species which are the outliers or transitionary links connecting two different orders or families together; as, for instance, *Adella frischella* and *Molanna angustata*, connecting the Lepidoptera and Trichoptera; *Carex rupestris* and *Sesleria cærulea*, connecting the carices and the grasses, &c. But there remain a vast number of instances which cannot so be disposed of. On what principle are the clear-winged moths, and numerous two-winged flies, invested in the robes of bees? Kirby and Spence thought that the meaning or object of such imitations on the part of some of them (the *Volucellæ* for instance, which deposit their eggs as parasites in bees' nests), was to allow of their entering the nest without being discovered. This may be so; and at least in the case of *Asaphes*, of which I have above spoken, probably is so, although we know that the bees are not very easily deceived, as it is well known that they will not even allow intruders of their own species from a neighbouring hive to enter with impunity. The fact may be, however, that although we notice them repelling some intruders from a neighbouring hive, many may enter unnoticed, and in like manner some *Volucellæ* may escape detection while others suffer for their intrusion. But admitting it to be so, this only explains the purpose of their being furnished with this livery; it by no means explains the means or law through which they receive it. It may be that if the law (supposing there to be such a law) under which the bees received the creative impulse, and assumed their form and colouring, admitted an influence from the condition and character of their birthplace, the same influence might be extended with a similar effect to the *Asaphes* and *Volucellæ*, seeing that the creative birthplace might probably be the same as that of the other, as their actual birthplace in point of fact is. My idea as to this, however, will be better understood after the reader has perused the second part of this inquiry.

But even after accounting for the class of impostors which make some use or profit of their disguise, there remain others, such as the clear-winged moths, still unaccounted for, where not only the colouring of the body, but also the additional transparence of the wings, is had recourse to to complete the deception. Further, there is the case where the same colours similarly and sometimes bizarrely disposed, are repeated in different families of insects. On this point, I may observe, that when such bizarre markings, or similar dispositions of colour occur, it usually (although not invariably) happens that the insects bearing them are from the same country. There are, for instance, several species of Coleoptera from Old Calabar in which singularly distorted angular yellow marks occur on a black ground on the back (*Nesioticus flasopictus, Nyctiobates regius, Nyctiobates militaris, &c.*); and a *Lycus* from Jamaica bears exactly the same amount and proportion of colour as *Pœciloderma terminale*, also from Jamaica—a scarlet body with a bright ultramarine tail, and so on. Such singularities suggest two explanations: the one, the possibility of new species arising from hybridisation—and we scarcely yet know enough of the true affinities of the different families of Coleoptera to be able to say whether such hybridisation might be possible or not in the cases I refer to. Another speculation may be, that their creation took place under similar conditions and influence of locality, which indeed, so far as we know it, corresponds with the present habit of life of the different species alluded to—the larvæ of the *Lycidæ* being wood-feeders, like the larvæ of the *Longicornes*, and a similar conformity of habit doubtless also existing between the larvæ of *Nesioticus* and *Nyticobates*, and of other equally illustrative species which might be mentioned.

But let us now turn our attention to those resemblances which consist in an imitation of inanimate objects, or of such animate objects as, from their masses, may in one sense be looked on as inanimate. One very common phase which we find this assume, is a general similitude to the ground on which the animal lives. How closely the colour of our common hare assimilates to the benty braes in which its form is placed; and still more, how the colour of the arctic hare, the polar bear, and other animals which live in the frozen north, fits to their snow-clad land. What a near resemblance does the grouse bear to the heather in which it couches. How well the partridge accords with the general hue of the stubble. How difficult it is to see the ptarmigan beside the gray stones among which it sits. Turning to the water, what more complete match can be found to the sandy bottom of the sea than the back of the flounder or the skate; or to the muddy bottom than the back of the plaice? Look at the tree-frog, so delicately and freshly green, sitting secure among the leaves which he so closely resembles; or at the North American frog, which is found on walls covered with gray lichen, with which he so exactly corresponds, that if he would only sit still no one could see him. Recollect the speckled snakes and serpents, with their coloured patterns so exquisitely blended, and so exactly resembling the tangled herbage through which they glide. Look at the lizards, some of the most lively green, suited to the foliage of the trees on which they live; others gray, like the stones among which they run; or yellow, like the sand on which they bask. Let us not forget the chameleon—which, certainly, whatever be the cause (whether it be an accidental result from mental emotion, an instinctive impulse independent of its will, or a deliberate intention to produce the effect), does exhibit a most striking resemblance in colour to that of the substances on which it rests, and a more marvellous power of adaptation to the variations in their hue than perhaps any other living creature. Remember the crocodile or alligator floating silently down the muddy stream, so like the trunk of a tree, that the unwary animal drinking on the margin only sees the deception for a moment, when the tree suddenly starts into life, and the victim is hurried below the waters locked in the reptile's formidable jaws. These are examples taken from the vertebrata, but we find similar deceptions repeated, only more frequently and more carefully, in the lower classes of animals. In insects, this is more marked than in any other class. In beetles, the form the deception takes is very frequently that of a pellet of earth or stone (*Byrrhidæ*, *Ceutorhynchidæ*, *&c.*), sometimes even a piece of silver or copper ore (*Chlamydæ*). We see a small beetle creeping along a plant; we stretch out our hand to take it; at the slightest motion it drops to the ground, its legs and antennæ collapse and are clasped close to its body, and all that remains is a small object, often irregularly marked by prominences, which is so like other little pellets of earth as to be scarcely distinguishable from them, and we only recognise it again by patiently waiting in silence until the insect has regained confidence, when it gradually and timidly protrudes its antennæ and legs, and puts itself in motion. A very common deception among beetles, particularly among the *Longicornes*, which feed on wood, is to resemble the bark of the trees on which they feed, or, in some instances, the lichens which grow upon them (*Rosalia alpina*, *Prionus cervicornis*, *&c.*) The *Orthoptera* carry the resemblances they assume more to specific objects. The whole of that tribe partakes more or less of the hue of their dwelling-place. The leaf-insect (*Phyllium*) is an example which will occur to every one. The *Mantis* is only a shade less like a leaf; and some are green like a young leaf, others brown and withered like a dried up and decayed one; and in some we find that a change similar to that in the leaf itself takes place; they are fresh and bright green in their youth, but fall into the sear and yellow leaf in the same way and at the same time of year as their prototype. A genus of locusts (*Eremobia*), which is confined to sandy deserts (Arabia, Egypt, Algeria, Sahara, &c.) furnishes another very striking example. The locusts found in less arid regions have the colours vivid, the upper wing brown or green, and the lower often bright blue or red. In the desert species, the body and upper wing are subdued to the same tone as that of the sand of the desert, and the lower wing has faded into a pale pink; and this concordance of colour exists in all the *Eremobias* or desert-livers which have yet been described, the pale pink colour of the under wing only being in one or two species replaced by a similarly faint shade of yellow, which is obviously equally well adapted to the purpose of harmonising with sandy ground, and possibly indicates a yellower tinge in the sand of the particular district which those so coloured inhabit. The orthopterous insects, known as walking-sticks, exhibit an imitation of another character. Their resemblance to dried sticks and straws is most perfect; and their long awkward-looking legs, sprawling in every direction, add to the deception. In their last and complete stage, they have little wings like dried leaves; and in some species, imitations of dried leaves, or rather of broken portions of dried leaves, are also appended to their legs. In the *Lepidoptera* (more especially the moths), we find not less resemblance to surrounding objects. So much is this the case with them, that few can be found which are not so provided. The great majority pass the day in quiescence, resting on the trunks of trees, under leaves, or on rocks, walls, or stones; and so exactly do they resemble the object on which they rest, that even when we see them fly from one spot to another, and alight under our very eyes, few but an entomologist would detect them. The pattern of speckled gray which generally composes the upper wing, although marked enough when displayed in a cabinet, is in reality a marvel of imitative skill. If any one will take a walk round a country house, and look carefully along the walls, he will probably see a number of moths resting on them, every one so ingeniously deceptive that he will readily admit that he has probably overlooked the half of them. The green and brown hues, imitative of every variety which foliage assumes, speak for themselves, and I therefore pass them over; merely repeating, that the number of insects so coloured is beyond calculation. But it is not only in the perfect state that these animals exhibit this persistency of imitation ; their larvæ are, if less universally, not less strangely and successfully metamorphosed. Look at the larva of the peacock-moth, and see how exactly the general ground-colour harmonises with that of the young buds of heather on which it feeds, and how closely the pink spots with which it is decorated correspond with the flowers and flower-buds. I should say that one half of the caterpillars of moths and butterflies are green, closely resembling the hue of the leaf on which they feed ; and when a part of the body only is exposed to view, the resemblance is often restricted to that part, as may be seen in the larva of one of our tiger-beetles (*Cicindela campestris*), which lives in a hole, from which its head and thorax alone protrude ; and these are of the same green as the perfect insect, while the rest of the body is of the usual whitish-yellow of a grub. There are few gardeners who have not in their time been surprised to find that they have taken a fleshy caterpillar between their fingers when they have thought to break off a dead twig ; the geometers, or loopers, having the strange habit of stretching themselves out stiff and stark like a twig on the shrub or tree on which they feed ; and as, like a vast number of the insect tribes, each is pretty much confined to one plant, or family of plants, for its food, we find that the resemblance is carried out to the extent of making the insect resemble the twigs of the particular plant on which it feeds, and, to add to the deception, is embossed at suitable intervals with one or two eminences, made to imitate the buds on the side of the twig.

Among the denizens of the waters we certainly do not find so many instances of disguise ; still we are by no means without examples. In one small crustacean, *Caprella phasma*, which in these days of tanks and vivaria must be familiar to many of my readers, exactly the same habit is repeated which we have found among the loopers. It is a linear animal, about half an inch in length, semi-transparent, and of the same colour and consistency as a zoophyte, such as an *Antennaria* or *Plumularia*. If put alive into a tank containing a bunch of these zoophytes, it fixes itself among them by its hind legs ; and stretching out its body, and every limb and joint, as stiff and rigid as iron, it requires a careful examination to be able to detect it. Among the marine annelides, too, we find the hue of the green, purple, and red sea-weeds exactly reproduced in *Nemertis* and *Planariæ*, &c. The mollusca do not furnish such striking instances of resemblance to particular objects—the amount of concealment shown in the sea-shells being chiefly confined to veiling the glowing colours of the shell under a coarse yellow epidermis, which corresponds in colour to the bottom of the sea on which they lie. Not that we are absolutely without instances of the place of life correspondingly affecting the colour, as for instance in the *Patella pellucida* and the *Patella cœrulea*, the former of which is dark, and of the colour of the frond of the large *Laminaria*, on which only it is found; and the latter, which is confined to the stalk, is paler, and of its hue:—an instance, however, on which I put no weight, because it may be pled that both the *P. cœrulea* and *P. pellucida* are the same species, modified by food; and where we have an *embarras des richesses* in the choice of unchallengable illustrations in other classes, it is unnecessary to burden ourselves with the defence of doubtful examples in this. Of the lower animals, the *Actiniæ* furnish some good examples of disguise. I can hardly suggest a more perfect one than *Actinia troglodytes* in a sandy pool, its tentacles being so exactly marbled like a sandy bottom, that the pool may be paved with them all expanded, and yet the casual observer—ay, more, the attentive but uninstructed eye—never see one!

The illustrations which I have thus far made use of are drawn from animals exhibiting these disguises permanently, and without variation; but there is another class of imitators which I must not pass over, those which have as it were more than one disguise in their wardrobe—one for summer and another for winter; as, for instance, the Alpine hare, the stoat or ermine, the ptarmigan, some grouse, &c.; and this a disguise which is made more or less perfect according to the severity of the winter, which is the accompaniment, I shall not say cause, of the change; for in species which constantly inhabit snowy regions, we find that the farther north we go the whiter they become—in the extreme north sometimes becoming replaced by a different species, wholly and permanently white; as in the case of the Arctic hare taking the place of the Alpine hare—both white, but the Arctic of treble-distilled purity compared with the Alpine. In other instances, the change is not dependent upon the seasons. In fish, it is a familiar instance that the colour varies with the colour of the waters. The trout in clear streams and lakes is silvery white; in peaty waters, dark. The Lochnagar trout, for example, where the waters are dark and peaty, is nearly black on the back. The flounder, which exactly resembles the ground over which it hovers, changes its hue in an amazingly short time, on passing from one bank to another of a different colour. Anglers who use minnows for bait know that if they are put in a light-coloured vessel they become pale; and if they be transferred to a dark one, they will become dark in the space of an hour. A similar though more permanent alteration takes place in various birds. Colquhoun, in his entertaining volume, the "Moor and the Loch," says, "In the low corn districts, such as Lanarkshire, Renfrewshire, and the Border counties, the grouse are of a very light brown, borrowing a tint from the stubbles on which they delight to feed." "Forty or fifty are often taken at a time (by snares) during the period between the corn being cut and carried. All these birds are so light in their colour as more nearly to resemble partridges" (p. 112). And we find the converse of this to take place in partridges. "These moor partridges," he says, "which spend much of their time in the heather, are of a darker colour than those of the Lowlands." (Foot-note, p. 113.)

Do any such resemblances as we have been speaking of obtain in plants? They are comparatively few certainly; but in some of the lower plants they certainly do exist. The crusted lichen often bears a close resemblance to the rock on which it is found, and the olive-tinted fucus to the wet reefs which it covers; but the examples are few in plants, and, what is not to be overlooked, we can see no purpose for their existence.

The hasty glance which we have thus cast over animated creation sufficiently shows three things—*first*, that the most perfect imitations of inanimate objects do occur; *second*, that this is not a rare or exceptional thing, but one found in every class of animals, and in some found so frequently as to be the rule, and the want of it the exception; and, *lastly*, that so apparent is the means to the end, that it is plain that there is a purpose in it, and that that purpose is the concealment of the animals that bear the disguise. Now, how does this come about? Is it the result of any general law; or must we be satisfied to say that these disguises have been given by Providence for the purposes of the preservation of His creatures, and rest there? I apprehend not. This is not the mode in which the Creator carries out His ends. It is indeed very easy to say, that a special exercise of the creative power has been used in these disguises in every separate instance; but if it is so, it is different from all we know of the other workings of Providence. A great law in all of them is set in motion, which, while regardless apparently of minor details and individual interests, yet, by its general working, most efficiently provides for them. Through the operation of such laws elsewhere, we see everything harmoniously and necessarily assuming its place and its form; and we cannot doubt that such a great law also exists in this matter.

What is this law? I cannot tell; but it has occurred to me, that the law may perhaps be found somewhere in the direction of an analogous force to the great law of attraction, otherwise called gravitation. Like draws to like, or like begets like. We have seen that in all the instances to which I have referred the external appearance of the animal bears definite relation to the appearance of the soil on which it lives, or the objects which surround it. It would appear as if there were a *genius loci*, whose subtle and pervading essence spread itself around, penetrating and impregnating the denizens of the place with its *facies*—possibly only affecting some, the conditions of whose entry on existence render them more liable to receive its impression than others; more probably affecting all, some more and some less. How this mysterious influence may operate, it may be bootless to inquire; but is it unphilosophical, or inconsistent with the simplicity and grandeur of nature, to suppose that one great idea should contain the elements of the laws which regulate all the different constituent parts of the created world? that as by attraction the material particles forming the solid globe gravitate to each other—as by the action of chemical affinities a like law regulates their intermixture—so a law of attraction should operate in the immaterial world of thought, and influence the phenomena of organic creation?

Assuming, then, as I think we are entitled to do, that some such law does exist, it will not be an unfair test of its truth to inquire whether its operation, like that of all other such laws, extends to all alike; whether its influence is extended both over the just and the unjust, over the rapacious and the inoffensive; or if it has only a partial bearing, confining itself to the weak and the defenceless, as a means of escape, in the absence of means of defence. So far as this goes, it bears the test sufficiently well. The crocodile receives the impress of its laws as well as the chameleon; the Polar bear or Arctic fox as well as the Arctic hare; the Mantis, which preys on other insects, as well as the leaf-insect, which feeds on leaves. In the one case, the deception is applied for offence; in the other, for defence. And if we remember that the numbers of those preyed upon must necessarily infinitely preponderate over those which prey upon them, we shall not be surprised at a much greater number of creatures using the disguise for protection than for offence.

Another test which may be applied is more difficult of answer. Looking at it as a law, ought it not to operate by itself, independent of assistance from the animal itself;—that is, if an animal assumes a disguise, ought not the disguise to be complete, pure and simple, without the aid of contrivance or trick on the part of the animal? A beetle, for instance, is exactly like a pellet of earth or a seed, but only so when its limbs are all contracted and placed close to its body. In addition to the resemblance, we must have here the instinct of the animal to throw itself into its proper attitude to produce the disguise. True, the natural instinct of fear leads it to shrink into itself; and we may look upon the resemblance as merely an accidental quality which is brought into play by this instinct. But this hardly seems to meet the difficulty. Two concurrent conditions are needed to produce one effect; and we can scarcely hold that the same law which in other instances produces the effect by its sole action, also, unaided, produces the effect resultant from a double action.

Were such a law of affinities or attractions as I have suggested to regulate the external phenomena of creation, we might expect that, as we do in chemistry, so here we should find many affinities opposing each other, and that the attraction of the colour or appearance of the soil, or habitation of the species, would only be one of many elements by which the conditions of its form and appearance would be determined. This would meet an objection which might otherwise be raised against my suggestion—viz., that on many animals there is no indication, or at least no apparent indication, of an affinity in appearance to the character of the place in which they are found. True, the answer may be likened to that used by the phrenologists, who, when some man with all the good bumps commits an atrocious crime, explain the anomaly by telling us he had counteracting bad bumps. The answer, by force of association, rather approaches the ridiculous; we are inclined to look upon it as a quirky evasion, but this is owing to its misuse and exaggeration; in sober earnest, it is so only when misapplied. Every one of us, if we ask our own conscience, must admit that we have a constant fight going on between our good and our bad inclinations; and the same struggle between opposing qualities, between opposing habits, and opposing laws, is to be seen in every branch of science.

Should my view appear of a nature tending to throw light upon the phenomena of creation, I apprehend it will not be found of less weight if I go a little further, and offer to extend it to the phenomena of life (as in contradiction to the phenomena of creation). These are by no means the same thing. If applicable solely to the phenomena of creation, my suggestion might apply to all such cases of permanent similarity to the *solum in quo* as I have adduced; but could not, or at all events could only by a more complicated and indirect route, apply to cases of temporary or seasonal variation. No original law of attraction by similarity could regulate the appearance of the variable hare at its creation, because such attraction could only affect it once;

and if the hare were once created white or brown, its creation is over, and further change through that law at an end. But if we look upon the rule as subsisting not only at creation, but all through life, and as a great law or influence affecting life, then the subsequent changes of the hare from brown to white, and back again, are in accordance with it. The greater whiteness of all animals at the Poles is explainable; we perceive under what law the fish or the bird assumes the hue of its feeding-ground, although we remain in ignorance of the *modus operandi* of the law.

There is yet a further step, which the advocates of a modification of species might not hesitate to take—viz., to abandon the idea altogether as a law of creation, and confine it to a rule of life. Combined with a belief in the modification of species, it might help us to see how, through its influence, so far as outward appearance is concerned, an Alpine hare might become the Arctic species; how the desert locust might be derived from the common one; how a moth like a weather-beaten, lichen-encrusted rock, should proceed from one from the woods, and so on. There may be truth in the idea that changes in species do arise from the modifications of climate, food, &c.; but I confess that, in the present state of my information on the subject, I am not inclined to look upon such instances as the above as cases where such modification has taken place, but rather as instances of the exercise of the original law I have assumed, or of some other such law of equivalent force and application; and would hold, that while such law may be exercised co-ordinately and simultaneously with one producing a certain amount of modification of species, it cannot be substituted for such modifying principle; still less can the instances of its operation be adduced as proofs of the existence of that principle.

As I have already said, the fact of a great family resemblance subsisting among all the creatures belonging to one continent and its dependencies, although not properly falling under the head *disguise*, must not be lost sight of in considering this question, and would seem to have a special bearing upon this phase of it. That such great regional resemblances exist, every naturalist knows. Contrast the deep and gloomy

Javanese forests, with their gorgeous epiphytes and poisonous-looking plants, with the fir-clad alpine scenery of Norway—the arid plains of Africa with the beauty of our West Indian Islands—the strange Australian scrub with the Tartarian steppes—and see how distinctive and marked is the general character of each; and not less marked in general effect than in individual detail. Show an experienced naturalist or botanist a new plant or animal, prating nothing of its whereabouts, and yet ten to one he will correctly tell the country from which it has come. There is an undefinable facies about them which enables the practised eye to allot to them their proper station, as the detective officer is said to be able to tell the felon, meet him in what company he may; and this family, or rather regional facies, extends around the particular country as the air of its climate envelopes it—as, for instance, in the Gallapago Islands, which lie nearest to America, although 500 or 600 miles distant from it, it is found that although the animals are for the most part distinct, they yet bear the American facies.

Now, although we find this regional character so well marked as I have stated, it stands to reason that all are not equally well defined. Where the contrast in climate and condition of life is great, the difference in organic productions is great likewise. The contrast is much greater between the fauna and flora of this country and West Africa than between those of this country and North America; and of course the converse is true likewise. Where the conditions of life are similar, the animal and vegetable products, although distinct, are found to be similar also. This gives rise to that most interesting class of plants and animals known as representative plants and animals.

But this is a word which is used with two significations, only one of which is properly applicable to my present inquiry. One kind of representative species (and that is the kind with which I am not at present dealing) is where a specific function is allotted to widely different species, in countries whose climate and conditions greatly differ. For example, the scavenger duty, which in cold countries is mostly performed by the burying beetles, is in tropical countries performed by heteromerous beetles, a totally distinct tribe. These represent each other in their functions, although not in their form or character. The representative species with which I have to deal are those which are found, in similar countries, replacing each other both in form and function. As in plants, the *Cacti* of South America replacing the *Euphorbiæ* of South Africa, the *Epacris* of Australia taking the place of the heath of the Cape, &c.; in insects, the *Eleodes* of California taking the place of the *Blaps* of Siberia, the *Eucrania* of the deserts of Northern Patagonia replacing the *Ateuchi* of the deserts of the Old World; in reptiles, the crocodile of the Old World superseding the alligator in the New; in birds, the ostrich in Africa represented by the rhea in South America, and the emu in Australia; in mammals, the leopard represented by the jaguar, the camel by the llama, and so on.

Now, it is to be observed that this class of representative species is only to be met with when similar conditions of life exist in both countries. The ostrich, rhea, emu, for instance, find the climate and the extensive plains suited to their structure and mode of life, in all the three countries where they are found; they are not to be met with in mountainous countries; the prehensile tailed monkies, the agile squirrels, &c., are only to be found where there are trees; and so in every other instance. Would it not, then, appear that the general character or facies of the animals in different regions is the result of some special influence or condition in the country peculiar to each? and if we adopt this view, must it not follow as a corollary, that, where the conditions are the same, we should see the same facies appearing in each. Now, in point of fact, in those cases where the conditions approach to the same, we see enough to show it to be probable that it is so. I would go further, and say that there is good ground for holding that, were they really *identical*, we should see the same facies reappear. Let me illustrate this by two or three instances showing the effect of different degrees of nearness of condition. Compare the United States with Europe: the conditions are similar but distinct; so is the fauna. Compare the salt deserts of the Caspian with the salt deserts of the Mormons: the conditions are here more similar; and recent discoveries tell us that the facies of the fauna of these two countries is singularly alike. Compare the Gold Coast, Gabon, &c., and a portion of the coast of Mozambique. Among the Coleoptera, we find Goliaths in both, and many other species allied to species on the opposite side of the continent. They are not the same, but only belong to the same genera. Is this the result of intercommunication, and does it indicate the existence of a similar zoological belt extending across the country? It is mere conjecture to say, No; subsequent explorations must solve the question; but it seems in the last degree improbable that the climate of the coast should extend through the centre of the land. And, lastly, compare the fauna and flora of the Arctic regions of Europe, of Asia, and of America, where the conditions cease to be different, and we find that *there* there is no longer a separate facies—the type of all three is the same. The light in which I look upon these facts is, that where, in a similar latitude, we have similar conditions of existence, we have dependent upon and flowing from that similar zoological provinces, or separate, distinct, and isolated patches, having nothing whatever to do with each other, but, at the same time, possessing allied species; and this I maintain, not through intermixture of species, or by modification of species, or by the passage of the same species from one continent or one part of a continent to another, but by the same conditions of creation presenting themselves for the evolution of the creative idea. Of course, in saying this, I do not speak of identical species, which may have passed from one place to another by the ordinary means of migration or intercommunication. I speak of the general typical resemblance which we find in species distinct and peculiar to each region; and my idea is, that as close a general resemblance would have been found, although each of these regions of the continent of which I speak had been separated from each other by thousands of miles, irrespective of latitude and hemisphere (the same conditions, of course, existing in other respects).

In short, if we could find two countries exactly corresponding in every respect, however widely separate, I should expect to find, not, of course, the same *species* in both (for as we have different species in our own and every country, so we must expect to find them there), but species of the same families and of the same genera, and, more than that, species very closely allied in form and appearance. It may be said, it is easy to argue from an *if;* but I think here I am able to offer something more than an *if.* No doubt the northern Arctic shores which I have instanced are not widely separated from each other; but I am able to refer to instances of places widely separated presenting conditions as nearly as possible identical, and producing species correspondingly similar. The first case is that of the blind cave animals found in the caves of Europe and Kentucky. Here, in a nearly corresponding latitude, yet in position separated by half the globe, are caves extending for miles into the bowels of the earth; and far in the interior of these caves,* where impenetrable darkness and everlasting silence reign, living eyeless creatures are found—more particularly a number of different kinds of insects. The number of genera and different forms yet known is few, but the number of species is considerable—almost every freshly examined cave furnishing something new. Now, the extraordinary thing is, not only that, in the different caves in Europe, the new species found in almost every fresh cave belong to already known cave genera,† but that the species found in Kentucky also belong to the same genera. The first species discovered there was *Anophthalmus Tellkampfii*, which so exactly corresponds in form and appearance to the European species, *A. Schmidtii* or *A. Bilimekii*, that any one but an entomologist would say they were the same. *Adelops hirtus* and *Adelops Tellkampfii* have since been found; and, in their turn, they so exactly resemble the Carniolan species *Adelops Milleri* and *Adelops Freyeri*, that the same may be said of them. Here, according to my view, the creative influence, acting under the same conditions, produces the same results—that is, produces creatures as nearly identical as can be without being the same. Nature never repeats herself; and we could not, therefore, expect the very same. And the inference I draw receives a double confirmation from the fact, that a great many of the European caves may be viewed as thoroughly isolated, and as free from communication as the Kentucky from the Carniolan; and I know of instances in which the inhabitants of such caves are not specifically distinct—the Auvergne species from the Pyrenean, the Pyrenean from the Carniolan, and so on. I may therefore say, that in the case of these caves we have a subterranean region as distinct and well defined as any purely geographical region in the upper world. Whether we may find other species of *Anophthalmus* or *Adelops* in the caves of Australia, or in caves yet to be discovered in tropical countries, is another question, and one which would not necessarily much affect the view I have taken, even although the species which might there be found should belong to new genera; for, in the first place, there may be more genera yet to be discovered in Europe—(I have no doubt there are)—and already we know several European genera which have not been found in America, and the converse may in all probability be yet found to be the case with regard to America; so that the mere discovery of a new form would go for nothing—the rather that our knowledge of the fauna of the subterranean region is as yet too limited to allow us to say whether a new form bears its facies or not; and, in the second place, the temperature, or, what is still more likely to affect the fauna of caves, the degree of moisture, may be sufficient to constitute a different class of subterranean region. The European and American is a wet subterranean, the Australian may very probably be a dry one; but if it should prove not to have different conditions, I then should most confidently look for the occurrence in them of new species of the European genera, *Anophthalmus* and *Adelops*.

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* Schiodte says of the Carniolan caves, that the blind insects are not found until about two miles from the mouth of the cave.

† The cave genera are, with one exception, confined to caves;—the one exception is the genus *Adelops*, of which some blind species are found, not in caves, but in dark places, under moss, &c.

Another region, *sui generis*, may be adduced as possessing in a certain, though less marked degree, special conditions of life under which similar peculiar forms have been evolved in distant countries, viz.—the interior of ants' nests. As already said, entomologists have of late years found many Coleopterous insects, not met with elsewhere, in the interior of ants' nests. Some of these (the *Clavigeri*, the *Formicosomi*, *&c.*) are invested with the outward appearance of the ants among which they live; others have no special resemblance to their hosts; but the point to which I press attention here is, that the true ant-nest species are found in ants' nests, and nowhere else; that a peculiar facies (in many cases a peculiar structure) belongs to most of them, probably to all which exclusively inhabit ants' nests. The search into ants' nests for these exceptional Coleoptera has been carried on very keenly for some years past in Europe, but as yet little has been done in foreign countries. The little that has been done has furnished very interesting results, and results quite in keeping with the view I have been maintaining. Species of *Paussi* (a marked genus with an exceptional structure, and believed to be absolutely confined to ants' nests) have been found in Spain, in West Africa, at the Cape, at Natal, in the East Indies, at Hong Kong, in Australia, &c. Researches in North America have likewise shown resemblances to our European species in other less exceptional species found in the ants' nests there. We must remember, however, that such a habitat is less defined and restricted, and more subject to the intrusion of external influences than the other special regions of which we have been speaking. There is another kind of condition allied to the last, also drawn from the insect kingdom, which furnishes another instance in support of my position. I mean the singular coincidence of form and appearance shown by all the species of water-beetles, wherever found. The conditions of life in a piece of water vary little, and we see as little variation in the appearance of its occupants. Entomologists have had their ingenuity exercised in endeavouring to account for the presence of apparently the same species of water-beetle (for instance, *Colymbetes notatus*) in distant countries—in New Zealand and Great Britain; and I have myself suggested, that the eggs or larvæ may have been transmitted in the water-vessels of ships; but it may be that the species is distinct, only that the characters are too slight to catch our eyes. At any rate, there is no doubt about the fact, that water-beetles are so close in structure to each other as to allow of only a very few genera, the great majority belonging only to a small number of distinct forms; and the chief difference appears to depend upon their being carnivorous or vegetable feeders. Again, there is a small beetle (*Æpus fulvescens*) found on our shores half-way between high and low water mark, and which certainly passes more than the half of its existence under the sea. There are two or three others of similar mode of life, but I more particularly refer to this one. Its usual place of abode is between the flat strata or leaves of shale or other foliaceous rock; and it is provided with means of securing and enveloping itself in a sufficient supply of air to last during its submersion, so as to maintain life, and move about unwetted. On the coast of Chili, a similar small species (*Thalassobius testaceus*) is found living under the same conditions, and having the same facies as our *Æpus;* sufficiently distinct to be made into a separate genus, but a genus taking its place next to *Æpus.*

It may be said, that if my views on this point were really sound, we should in like manner find the fishes and mollusca of our own seas, and of those of Chili, &c., bearing the same type. But this by no means follows; for observe, in the first place, we are here comparing an animal created to suit an abnormal condition with those in a normal condition, and it might be expected that the abnormal condition would express itself more forcibly in any deviation of structure than the normal condition; and, in the second place, we are comparing land animals with sea animals, and we are not entitled to assume that the amount of variation in the condition of life necessary to influence the creative idea in the one is the same as that in the other; or that the same tests indicate the relative amount of variation in condition of life in both. On the contrary, we have every reason to suppose that the reverse is the case—namely, that the creative idea in sea animals was (or is) influenced by a less amount of variation in the conditions of life than in land animals. In all animals and plants, the phenomena of life bear a perfect relation to the phenomena of creation. As already said, the animal created in and bearing the typical facies of any geographical region, is suited to the conditions of life in that region, and thrives better there than anywhere else; and cannot live at all in regions which are much opposed to its native one. The reindeer could not live in the deserts of

Sahara, nor the lion in the snows of Lapland. In fact, we may say, that where we find one set of phenomena, we shall also find the other. If we find a region of which the flora and fauna are typically peculiar, we shall find that that flora and fauna is not suited, at all events not so well suited, to any other region. They may live in another region which has approximate conditions of life to their own, but will not thrive in it so well as in their own; and however similar the other region may be to it, they will never make their way in it in opposition to its own native fauna and flora, so as to usurp their place. If, therefore, in nature, we find a special fauna confined to a special district, we may assume that the same conditions which influence its restriction to such local habitat also influenced its original creation. Applying this to the question of the relative amount of variation in condition of life necessary to have influenced the phenomena of creation in land and sea animals respectively, we see that if this had been alike in kind and amount, we should have had the same facies in sea animals nearly all over the globe; because temperature and some other particulars, which will suggest themselves of their own accord to the reader (the great difference in the amount of which forms so important a condition of life on land), are so equal all over the sea that they could form no efficient barrier, were their effects no greater there than what they are on land. All the differences that we can detect between one region and another in the sea, would prove no barrier to the passage of most animals on land. We must therefore allow, that the inhabitants of the sea are either more susceptible than land animals (and *that* both in the phenomena of creation and of life) of the differences in the medium in which they live, or else that there exist in that medium other elements influencing the inhabitants of the sea, which on land do not affect animals, or which we cannot appreciate. If we admit this, we can have little difficulty in suggesting some of the specialties which may have given rise to different marine regions. The difference between the extent of sea and dry land in different parts of the globe, the greater depth and wider extent of the sea in the south, the different distribution of land, the different ingredients of which the shores are composed, combined with temperature, depth, climate, weather, currents, and possibly even some small difference in the relative proportions of the chemical constituents of the sea itself,—all must operate in affecting the phenomena of life and creation; and it is through the influence of such particulars that we can explain the differences of type in the marine fauna and flora of seas separated from each other only by a few miles of land—as, for instance, the Pacific Ocean and the Gulf of Mexico at the Isthmus of Panama, or the Mediterranean and Red Seas at the Isthmus of Suez.

If there be any such law, it must have been in operation during past geological epochs as well as at the present time; and wherever we can find any stratum containing animal and vegetable remains, marked by a similar facies to that of the animals and vegetables which we now find in any existing region, surely we may draw from them a tolerably correct inference as to the climate and nature of the country in which such bygone forms once lived. If we find, for example, that the fauna and flora of the oolitic period bears a certain resemblance in facies or type to the present fauna and flora of Australia, we may assume that those regions of which the oolitic strata are the debris bore in their time a corresponding amount of resemblance in physical condition to the present state of Australia.

So much for similar conditions of locality producing similar plants and animals. It remains to consider what are the conditions which so influence the creative or vital powers, and how they operate. Can my suggestion of the attraction of resemblance to the character of the birthplace have any bearing here? Its application is certainly less apparent, but, I should say, may still exist. We must remember that there may be, and no doubt are, many points of resemblance which our faculties are unable to appreciate, but which yet exist; and many which, although we cannot nominate, we yet are sensible of. Most men have some idea of their own of the general character of the different continents—something indescribable, but which yet associates well, in their ideas, with the plants and animals which they know to inhabit them. The essential element of some such inappreciable character may be that which, communicated to the whole animal and vegetable creatures of a continent, gives them that general empirical resemblance which we admit, but cannot embody in words or explain to another.

Notes on the Aurora of the 28th August, and several subsequent nights, as observed at Lunenburg, Massachusetts, Lat. 42° 35′. By Professor William B. Rogers. With a Plate.

We place on permanent record in our pages the subjoined very interesting description of the remarkable *Aurora borealis* of the end of August last, by our esteemed correspondent, Professor William B. Rogers of Boston, United States, in the hope that its publication may assist in the identification, or the co-ordination at least, of some of the phenomena with those witnessed at the same date in other quarters of the globe. Recent intelligence indicates that an auroral condition of the atmosphere, of unwonted brilliancy, prevailed at the same epoch throughout an unprecedented extent of the earth's surface, being visible on the west side of the Atlantic as far south as Havana, and on the east side in many parts of Europe; how far to the west and east, we cannot at present report. What is more worthy of note than even its occurrence within the tropics, is the fact of its appearance, with identity of date, in the southern hemisphere, in Australia, at our very antipodes. A diffusion so nearly world-wide of a disturbance in the electric equilibrium of the earth's atmosphere seems to lend to this aurora the character and interest of a cosmical phenomenon, and to suggest that philosophers must look for its exciting cause either outside of the earth altogether or deep within it. Nothing will so tend to the discovery of the cause, whatever it may have been, as a careful comparison and analysis of the aspects and phases of the aurora, observed simultaneously at many remote localities and faithfully described. The reader will find an account of the phenomenon as it was witnessed on the 28th and 29th of August and 1st and 2d of September at Havana, in an abstract of a paper read before the Académie des Sciences, Paris, by M. Poey, on the 17th October, and printed in the journal called *L'Institut;* and he will see a mention of an *Aurora*

Australis on the 28th of August, &c., at Victoria, in the *London Illustrated News* of the 19th of November.

Letter, Professor W. B. Rogers to Professor H. D. Rogers of Glasgow.

"MY DEAR BROTHER,—The close of the past summer was marked by a succession of auroras, which, for number and magnificence, have rarely been equalled in this latitude. The favourable position of our hill, commanding the whole horizon, led me to note in some detail the varying phases of the phenomenon on the night when it was most brilliant, and to mark its recurrence at other times throughout the period of its continuance. Knowing that you will be curious to compare these observations with the effects which, as I learn, were observed at the same time in Great Britain, I send you a copy of my notes, compiled from memoranda taken at the moment of observation.

"This memorable period of auroras was ushered in by the superb display of the night of Sunday, August 28–9, of which some details are given below. After this the phenomenon was repeated with more or less splendour for the eight following nights, that is, until the night of Monday, September 5–6. Two of these later displays, those of Thursday and Friday, were scarcely inferior in beauty to the first, while that of Friday, in some of its features, was the most interesting of them all. The uniformity with which certain of its phases recurred, and the marvellous exhibition of pulsations which marked it when most brilliant, are described somewhat minutely in the accompanying notes. In all the lesser displays, as in that last mentioned, the formation of the obscure segment in the north, with its marginal bow of light, was the prominent phenomenon; and the shooting up of streamers from this arch was accompanied by an exhaustion of its light and a breaking up of the dark space, which, after a pause, was reproduced as before.

"For several days during this auroral period, the telegraph wires were observed to be charged; and so powerful were these waves or currents of the induced electricity, that, on some lines, they enabled the operators to correspond without the use of batteries. Between Boston and Portland, the line was worked for two hours by the aurora alone. A similar result was obtained between Braintree and Fall River, between Boston and Fitchburg, and on the vast stretch of wire from Pittsburg to St Louis, as well as on other routes, having more or less of an eastern and western direction.

"*Aurora of the night of August* 28–9.

"A few minutes before 8 P.M. of the 28th, my attention was called to an appearance resembling a number of elongated, thin, slightly luminous clouds, collected chiefly in the eastern and western quarters of the sky, united by others extending over head in nearly parallel directions. These were quickly replaced by converging columns of the same vapour-like aspect, mingled with more transient luminous streamers, clearly showing the auroral character of the phenomenon. Throughout most of the northern half of the sky the stars were dimmed by what seemed to be a luminous haze, which in some places quite eclipsed their light, and which itself glowed changefully with a golden and crimson colouring. These tints, so indescribably delicate and rich, were brightest in the eastern and western quarters of the sky, where they continued to appear in varying flushes overspreading a wide space, until near the close of the first great display of the evening. In the earlier stage, the obscure space on the northern horizon had not assumed the usual arched form, and was sufficiently translucent to show a few flaky clouds floating within its confines.

"At 8.20, this dark space had become more opaque, and had moulded itself into a symmetrical arch, bounded by a broad luminous band. Quickly groups of streamers shifting from place to place arose along the arch, and then appeared in a long array flashing from nearly every point far up into the sky, while similar luminous columns, spreading along the horizon beyond the east and west points, and extending upwards, united with the long beams dimly stretched over from the north to mark out a second obscure segment in the opposite or southern part of the heavens.

"As these luminous columns rapidly brightened and advanced, the southern segment, which they delineated, grew more distinct and regular, forming in a short time a great arch, whose dark area embraced a much wider space and greater altitude than its counterpart in the northern quarter. From this time the luminous columns rising from the southern arch continued to increase in brightness, while those on the north grew fainter and less regular, and the arch from which they ascended relapsed into a changing shapeless form.

" At 8.50, the *southern half of the heavens had become the seat of the most striking phenomena*, and so far outshone the north, that the display might well be called an *Aurora Australis*. Here was seen a vast dark arch, embracing about 130 degrees of the horizon, and rising to a height of 50 degrees, bounded above by a broad luminous zone, from which the intermitting, far-flashing columns of silvery and golden light seemed to emanate. These gradually encroached upon the arch below, which, as it became depressed, drew with it a border of increasing brightness. At the same time, the luminous columns lengthening upwards began to mark a point of convergence south of the zenith, corresponding to the upper pole of the dipping-needle. Sometimes two, sometimes three or four would flash up to this part of the heavens, and, lingering for a moment, would seem to unite around the pole, often leaving, as they retreated or dissolved away, irregular flakes of light encircling the polar point.

" These phenomena continued with little general alteration for many minutes, the southern arch slightly declining in height but not in brilliancy, and another but less luminous arch forming in the north, and sending its flashing streamers towards the magnetic pole, whither also continued to converge the golden and crimson beams emanating from the eastern and western quarters of the horizon. After a time, these converging flashes began rapidly to increase in number and brilliancy, encroaching more and more upon the southern and northern arcs of light, until, at about 9.15, the latter were entirely obliterated amid the suddenly increasing splendours that overspread the whole sky.

" At 9.30, the display attained its highest magnificence. The dome of the heavens was hung around with white, and golden and rose-tinted streamers converging from all quarters towards the magnetic pole, and uniting there in a circle of continually varying brilliancy and colour. Over the glowing stripes of this marvellous pavilion, there came broad flushes of the richest crimson light, at first at intervals, and then more permanently, until it suffused all the upper part of the sky and the whole southern quarter except a narrow space next the horizon, where only a few faint streamers were to be discerned.

" These phenomena continued with little abatement of splendour for about half an hour, the light diminishing somewhat in the lower part of the sky, especially towards the north and south, but still glowing in changeful beams with broader flashes of crimson and gold over a wide space around the magnetic pole.

" At 10.15, the general illumination had ceased, but broad masses of rose and yellow light still hung over the eastern quarter, and less distinctly in the west, while pulsating beams continued to play at intervals around the magnetic pole, and to flash over other parts of the sky. At 10.30, nothing remained of this wonderful spectacle but a faint auroral arch low down in the north, accompanied by a few dim streamers.

" At this time I discontinued my observations; but from the reports of others, it appears that the phenomena recurred in great splendour between one and two o'clock in the morning (29th), when the crimson colour was particularly remarkable. Still later, at 3.30, I saw a fine auroral arch in the north, with a long arra yof streamers rising from it.

" During the evening, when the illumination was at its height, the light was so great, that several of my friends, standing out on the lawn, *were able to read with ease the ordinary small print of a newspaper*. As the evening advanced, the breeze from west by north, at first feeble, became quite strong. At 9 P.M., the barometer marked 29·5 and falling, and the thermometer 56°, an unusually low temperature for the season.

" *Aurora of the night of September* 2.

" Early on the day of the 2d, the wind became strong from west, with a light haze. Barometer 29·2, lower than for many days. Temperature at 10 A.M. 60°. As the day advanced the wind veered to north-west, the air growing clear and showing scattered cirrus and cirro-cumulus clouds. A clear sunset was followed by a peculiar greenish and purplish light extending round the horizon, even beyond the north. Over the north-east quarter, the air, to the height of thirty degrees, had a dark opacity, unlike that caused by mist or cloud, but rather resembling a shadow, and yet having the effect of arresting the light coming from beyond.

"At 7.30, an irregular, obscure space began to form along the northern horizon, and a faint flush of whitish light to appear at a point a little north of east.

"At 7.50, a faint arch of white light made its appearance, resting on the horizon a little north of the east and west points, and culminating some distance below the pole-star. This continued to rise until 8 P.M., when its apex was within a few degrees of the pole. At this time two broad but faint beams extended from the ends of the arch towards a point a little south of the zenith, forming a second but much fainter arch. Within the northern arch appeared a narrower luminous band, concentric with the exterior curve, and separated from it by a dark opaque space. Between this and the horizon there was an obscure tract traversed by faint, vertical beams, which terminated at the inner luminous arch. (See fig. 1, Plate III.)

"At 8.10, the arch had declined to about two-thirds the height of the pole, but was very symmetrical and distinct, displaying the inner concentric band and some streams in the dark space below, but none rising from the east and west extremities.

"At 8.15, a long, bright streamer shot out from the west end, while the other extremity became suffused by a broad mass of light, giving forth another streamer. The two concentric arches, verging into the luminous space, still retained their regular form throughout the rest of their length.

"At 8.20, the inner luminous curve had vanished, and the eastern end of the arch was replaced for a considerable distance by a broad mass of light, giving origin to several streamers. (See fig. 2, Plate III.) It now slowly descended and faded, until at 8.35 it began again to rise and to acquire more distinctness.

"At 8.40, the top of the arch reached nearly to the pole-star, the east limb had resumed its regular form, and the west limb was replaced by an irregular space without distinct streamers. but displaying an intermitting or pulsating light.

"At 8.45, the arch had declined in height, and was so faint as to be hardly visible except at the flanks.

"At 9 P.M., it had grown luminous throughout, had ascended a little, and developed a faint inner arch, while occasional streamers rose from the eastern end, now broken and irregular. Soon they vanished, while others shot up from the western extremity. The upper arc became irregular from the effusion of dim waves of light upward. (See fig. 3, Plate III.)

"At 9.7, the inner arch below the broad irregular zone of light had become strongly luminous throughout, forming a narrow concentric curve, extending to the horizon on both sides. The apex of the outer curve was less than half the height of the pole. No streamers were visible within or near the arch.

"At 9.20, the inner arch had vanished, the upper arc at the same time becoming higher and fainter by diffusion, while dim streamers appeared on the horizon near the western extremity. Presently a low, luminous segment showed itself on the horizon beneath the arch; the latter now resolved itself into an array of bright streamers, with equidistant shadowy spaces between them, to which was quickly added a similar array of short streamers beneath. (See fig. 4, Plate III.)

"In a moment the streamers began to shoot upwards to a great height, and to exhibit, first in the eastern quarter and then at various points, flashes of crimson and golden light, while near the horizon at one or two places stripes of prismatic colour gleamed for a few seconds, looking like bright fragments of a rainbow.

"At 9.30, the streamers had extended and grown brighter, while the low luminous segment, diffusing itself upward, had merged into the outer arch, which now reached nearly to the pole-star. At this moment the arch began to send off *successive waves of light, rapidly following one another* towards and beyond the zenith, some of which could be distinctly traced as great continuous curves, resting on the eastern and western points of the horizon, and moving as if they revolved about a diameter connecting these points.

"At one time two of these vast arch-like waves were seen at the same moment, the one following the other at no great distance as they swept upwards and across the zenith.

" In a few seconds, however, this phasis of the wave movement gave place to more rapid and seemingly broken pulsations, flitting upwards in close succession through the northern, eastern, and western quarters of the sky, and visible, though less distinctly, in the south. This wonderful appearance, though difficult to follow in its details, exhibited everywhere a convergency of the lines of motion towards a point considerably south of the zenith, and seemingly correspondent to the upward direction of the dip-needle.

" At one moment bright streamers shooting from the horizon would meet at this spot, returning for an instant a nearly continuous illumination throughout their length, at another they glowed chiefly towards their upper part, where they united to form the auroral crown.

"When these luminous phenomena were at their height, every spot to which the eye was directed, except the southern quarter near the horizon, was traversed by quickly successive flashes of white, greenish, and pale roseate light, all seemingly moving upwards, and presenting, when viewed together, the appearance of alternate spaces of illumination and obscurity chasing one another along the great streamers converging towards the magnetic pole.

" These upward pulsations of light seemed to be a phenomenon distinct from that of the streamers themselves, which appeared to be *comparatively stationary*, though but indistinctly and partially visible.

"As this impressive effusion of light went on, the northern arch broke up into an irregular faintly luminous space, but quickly the low dark segment on the horizon expanded, and another arch began to be developed, partly within and partly above it.

" At 9.57, this double arch was complete. The pulsations were not observed in or above it, but continued in other parts of the sky, especially in the eastern quarter, where they seemed, as before described, to illuminate alternate spaces along the comparatively stationary streamers, which were only dimly discerned in their absence.

" At 10.3, the arch had become *triple* by the appearance of a third luminous bow within the dark segment. The uppermost curve now shot forth numerous streamers, and was quickly lost, leaving a double arch of great symmetry and brightness. In a few seconds short equidistant streamers formed in the dark spaces between the two concentric arches, and soon extending outwards, obliterated the external bow, leaving the inner one single with a superb tiara of radiations.

"At 10.17, faint beams were seen converging towards the magnetic pole, accompanied by occasional pulsations. The streamers in the north had nearly disappeared, and the space beneath had assumed the form of a dark segment of great height with a single luminous margin.

"At 10.20, the arch was very bright and showed prismatic bands near its western end, beyond which arose a broad roseate beam, extending more than half way to the zenith. Pulsations were seen within and above the arch, and also near the magnetic pole.

"At 10.30, the dark segment below presented three bright concentric bands. Above was a luminous arch wider than usual; and exterior to all, another but less brilliant one, from which numerous streamers seemed to arise. Again a superb flush of crimson light was diffused from the north-west, while prismatic colours made their appearance in the near extremity of the arch, accompanied by a renewal of the pulsations in that region. Meanwhile the inner and outer arches coalescing, became divided into a series of luminous prisms, and in a moment the pulsating movement extended over all the northern and part of the southern half of the sky. Innumerable waves of white, yellowish, and purplish light chased each other from every quarter towards the magnetic pole, while the crimson flush before referred to spread wider and higher from the west. This grand display was marked as before by a *breaking up of* the luminous arch in the north, where in a few minutes there remained only a dark segment near the horizon.

"At 10.45, this latter had risen a little, and resumed its luminous bordering; while faint beams, seemingly made visible by pulsations running along their length, were seen to the south of the magnetic pole, as well as in other quarters. At this time the margin of the dark segment extended over Capella, and quite extinguished its light.

"At 10.55, the arch became undulated, showing both streamers, while the pulsations became more numerous and brilliant in the various quarters in which they had been previously seen.

"At 11.5, the eastern part of the arch had given place to groups of rosy and golden-coloured streamers, rising high. Quickly the same change occurred in the other limb, and the arch was obliterated; flushes of rosy and greenish light alternated in the west, and faint converging beams were visible near the magnetic pole. The pulsating movement now ceased to be discernible. Here the regular observations were closed, but the auroral lights probably continued through the night, as at 3 A.M. of the 3d I saw a luminous arch, with streamers, in the north.

"On reviewing the various phases of this aurora, it will be seen that they recurred according to a somewhat uniform order of succession. First, the dark segment on the northern horizon took a regular arched form, and, as it rose, became bounded above by a broad luminous curve, at the same time developing one or more bright concentric arches within. The streamers, previously absent or infrequent, now shot forth from all parts of the luminous zone; and as these increased in extent and brightness, the upper arch faded away, as if it had expended itself in producing them. And now the lower arch took its place, to be obliterated in its turn by a like seeming process of exhaustion. At length, one of the grander effusions of light coming on, the whole arch was broken up, and the dark segment below was reduced to a shapeless mass. There then occurred a comparative pause in the phenomena, until the dark segment again took form, with its one or more luminous bands, and a like cycle of development was repeated."

Supplementary Remarks on the genus Galago. By ANDREW MURRAY, Edinburgh. With a Plate.

Since writing the paper describing the *Galago murinus*, published in last number of this Journal, I have received from my friend, the Rev. W. C. Thomson of Old Calabar, some interesting information regarding the habits of the genus which at that time I desiderated.

He tells me that there are two species found at Old Calabar—the one which I described as *murinus*, which is the smallest, and another about the size of a rat. He says, "Young ones of both species are brought to us about this period of the year (July 26). Mr Robb has a young specimen of the smaller species just now, and about this time last year I became possessed of one of the larger. It was a most interesting and amusing pet, not only quite tame, but manifesting strong attachment. I had it for about six weeks in my possession, when, unfortunately both for myself and it, it took a false leap into a water-barrel and was drowned. It was a very epitome of zoology, of the size and colour of a large rat; it had the tail of the squirrel, the facial outline of the fox, the membranous ears of the bat, the eyes and somewhat of the manners of the owl in its cool odd way of peering at objects, the long slender fingers of a lean old man, who habitually eats down his nails, and all the mirthfulness and agility of a diminutive monkey. It hated its cage at night, but delighted to leap among the bars of the chairs ranged purposely round the table for it. It could clear a horizontal distance of at least six feet at a leap; and whenever it fell, as during its short apprenticeship it often did, and from alarming heights too, it gave expression to its parenthetic chagrin by a rough sort of purring. It possessed the curious power of folding its membranous ears back upon themselves, and somewhat corrugating them at pleasure; and it appeared to me that the palms of its hands, all four, were endowed in some degree with the power of suction, such as the walrus is said to possess in perfection. I have seen it maintain itself in positions where the mere lateral pressure of its limbs appeared to be inadequate for the purpose; and I once applied it to the side of a cylindrical glass shade, of which it could not embrace so much as a third of the circumference, and sure enough it maintained its position for some time, gradually sliding down until it gave way. The palm was very much depressed, always clean and glistening, surrounded by five papilliform growths, those near the roots of the fingers serving as points of opposition to them, the fingers never closing beyond the palm." "Mr Robb had one of your species in his possession for a considerable while. It devoured grasshoppers and even the fierce *Mantis* greedily, as well as moths, little as it was; but I never saw mine muster courage enough to attack either grasshoppers or *mantis*, though nearly twice as large as Mr Robb's. No doubt mine would by and bye have become less particular and more daring."

In compliance with the wish of some of our naturalists who have desired to see somewhat more of the anatomy of this genus, I have added another plate (Plate V.), giving a view of the viscera of the species I described, at least of two of the most important parts of the viscera. One of these is the stomach and intestinal canal, in which it will be observed that, unlike those of other insectivorous animals, there is a rather long cœcum. This is of interest, from the circumstance that the absence of the cœcum in the insectivora has been attempted to be accounted for on the hypothesis, that had it been present, injurious consequences might have arisen from angular fragments of the elytra or hard parts of insects lodging in the cœcum and occasioning a fatal obstruction in the same way that in our own species death is occasionally caused by obstruction in the intestinal canal, originating in the presence of a fish bone or some trifling impediment of that nature being lodged in the cœcum. Here we have an undeniably insectivorous animal with a cœcum proportionally six times larger than that in the human species.

Another point in which the Galago differs from the normal character of insectivorous animals is, that it has only one superior *vena cava*. None of the insectivora, properly so called, have only one. I have added a sketch of the origin of the subclavian and carotid arteries, to show that there is a short innominate artery, in this approximating to the arrangement of the same vessels in the human species.

The lungs also correspond to the structure of these organs in the human species, the right having three lobes and the left two; in this respect also differing from the rodents, which have always more, and sometimes many lobes.

There are eight true ribs, three false ribs, and two floating ribs.

The dental formula is

$$i\,\frac{2-2}{3-3}\quad c\,\frac{1-1}{1-1}\quad p\text{-}m\,\frac{3-3}{3-3}\quad m\,\frac{3-3}{3-3}$$

I have had a little hesitation whether to say three præ-molars and three molars, or two præ-molars and four molars. The distinction between the posterior præ-molar and anterior molar is so slight, as to allow the formula to run either way without implying any great error in judgment in the describer.

In the more close examination which I have now given to this animal, I find a structure in the front of the upper jaw which I had previously overlooked, and which is not represented in the figure of the roof of the mouth which I gave in last number. It is two small orifices (as large, however, as the root of the superior incisors), situated in the middle space between the two incisors on each side, but a little behind their line. Their position suggests an analogy to Jacobson's vesicles in the horse; and on tracing their origin, we find that they lead to the nasal orifice, expanding before they reach it into a sort of sac, which appears to communicate by a narrow and short canal with the nasal orifice, in this respect differing from Jacobson's sac, which does not communicate directly with the exterior. To make this structure plainer, I have again given a figure of the roof of the mouth, in which these orifices are delineated, and also of a vertical section showing the form of the sac and its apparent communication with the nostril. I say apparent, because the tissues in my specimen are hardly in a sufficient state of firmness to allow me to speak with absolute confidence. The result appears to be, that there is here a communication between the nostril and the roof of the mouth, immediately and closely behind the symphysis, and almost in the line of the incisor teeth. What may be the use of this structure, I must leave to my Old Calabar friends to discover, contenting myself in the meantime with pointing out its probable existence.

Explanation of Plate.

1. Figure of body opened, showing—*a*, the stomach—*b*, the small intestines—*c*, the cœcum—*d*, the diaphragm—*e*, the heart—*f*, the innominate artery—*g*, the subclavian arteries—*h*, the carotids.
2. *a*, the roof of the mouth showing the orifices analogous to Jacobson's—*b*, vertical section of symphysis showing the sac from which the orifices proceed, and its apparent communication with the external nostril.

REVIEWS AND NOTICES OF BOOKS.

Christianity contrasted with Hindu Philosophy. By JAMES R. BALLANTYNE, LL.D., Professor of Moral Philosophy, and Principal of the Government College at Benares. 8vo, pp. 236. London: James Madden, 1859.

As a part of its educational system in India, intended for the enlightenment of the natives, the English Government maintains several colleges, which embrace both an English and an Oriental department. In these institutions, instruction in the English language and literature is afforded to such native youths as desire it, while they are at the same time taught their own vernacular dialects. In the Sanscrit department, the learned language of India, and its literature as contained in the various Sāstras, form the subjects of tuition. The Sanscrit colleges of Calcutta, Benares, and Poona contain a considerable number of native professors, who teach Sanscrit grammar, Hindu philosophy, law, mathematics, &c. Formerly, these native professors were left very much to themselves, to teach their own subjects in their own way; and no attempt was made to impart to the students any other education of a more enlightened character. Latterly, however, an endeavour has been made to introduce an improved system, and to make the Sanscrit students in some degree acquainted with English science and literature. The Calcutta Sanscrit College is directed by an enlightened Brahman; Dr Haug, a learned orientalist from Germany (formerly an assistant of Baron Bunsen in some departments of his literary labours), has just gone out to superintend the Sanscrit studies at Poona; and the author of the work before us, Dr J. R. Ballantyne, has for the last fourteen years presided over the Sanscrit as well as the English department of the College at Benares. He is a man well fitted for this duty; and his labours in the Sanscrit department have been carried on in the most liberal and enlightened spirit.

The natives of India, the inheritors of an ancient civilisation, cannot be properly treated as if they were a tribe of savages. Learned men, whose ancestors had more than two thousand years ago cultivated and methodised the principles of grammar, and had initiated various systems of philosophical speculation, which have been discussed and modified by the successive generations of their descendants, cannot be dealt with as if their minds presented a *tabula rasa*, on which their European instructors could inscribe any new notions they pleased. On the contrary, their minds are completely preoccupied and powerfully influenced by principles of thinking and reasoning derived from their own venerated writers, and grounded on the ancient sacred writings, the Vedas, which they regard as inspired. Any teacher, therefore, who would make himself thoroughly intelligible to such men, and would venture to indulge the hope of bringing them to take any real interest in the truths of European science, must study the systems in which they take a pride; must distinguish between the truth and the error contained in their speculations; must recognise and appreciate the former, while he attempts to overcome and neutralise the latter; and will act wisely if he takes the many truths which are embodied in the Indian theories as the starting-point and basis of the new truths, scientific or religious, which he wishes to communicate to the learned Indians. These are the enlightened principles by which Dr Ballantyne has been guided in his endeavours to introduce the science and learning of Europe to the notice of the students of Sanscrit.

In pursuance of this object, he has judged it expedient first of all to bring to light the principles of the six orthodox systems of Hindu philosophy, by publishing (with translations) either some of those brief Sanscrit treatises in which they are expounded, or the introductory portion at least of those more detailed aphorisms in which the different systems are authoritatively explained. Simultaneously with his labours in this department, which are still proceeding, he has been endeavouring to apply in practice the knowledge thus acquired. Finding that of the six systems of philosophy the principles of the Nyāya are the most reasonable, he has taken it as the basis of a Synopsis of science, which he has published in English and Sanscrit, for the purpose of initiating Sanscrit students in the whole circle of European knowledge, physical and moral. The Indian system in question does not, however, appear prominently after the introductory chapter, and the bulk of the work is devoted to a summary exposition of western science. This work has the great advantage of being not only accurately, but scientifically rendered into Sanscrit. The author's researches, aided by the skill and knowledge of the first-rate native scholars by whom he is surrounded, have enabled him to make use of the proper technical terms existing in the Sanscrit writers, in all cases where they have treated of the particular subjects under consideration. The translation is not a literal one, which would be of little value, and nearly unintelligible to native scholars. It professes to do no more than employ *equivalent* terms and expressions, and this it does in a peculiar idiomatic way. The book is thus cast in a peculiarly Indian mould.

The work named at the head of this paper, which obtained the moiety of a prize offered some years ago for the best refutation of the Hindu philosophy, and the best demonstration of the fundamental truths of Christian theism, may be regarded as a continuation of the "Synopsis of Science" above described. The position of the author, as head of a government college in which Christianity is not allowed to be openly inculcated, prevented him from introducing that subject into his Synopsis. In the present publication, that deficiency is in some measure supplied. It is a work of remarkable ability and interest, the production of a man gifted with great clearness of view and much speculative acuteness. We have here the first attempt which has been made to reason with the learned Hindus in the technical language of their own philosophy, the most essential parts of the book being accompanied with a version into idiomatic Sanscrit. In thus succinctly presenting the principles of our apologetic theology, together with the truths of Christianity, in the scholastic forms to which the Indian Pandits are accustomed, Dr Ballantyne has asserted a new claim on their attention, and has taken possession of ground which none of our professional missionaries has yet been able to occupy. The plan of the treatise is as follows:—

We have first an introduction, in which the author inculcates the necessity of delicacy and address in all cases where interference with long-established religious opinions is attempted, the duty of abstaining from rude assaults, and the expediency of paying attention to the learned class, and generally of practising conciliation. Proceeding on the opinion that speculative error may often be allowed to lie dormant, and be neutralised, or even superseded, by sound practical principles, without being directly redargued, the author adds the very judicious remark, that a primary refutation of Hinduism is not necessarily required for the propagation of Christianity. And yet the missionary should always possess such a knowledge of the tenets which he is seeking to overthrow as may enable him most effectually to adapt his lessons to the peculiar tendencies and wants of those whom he is seeking to convert. The writer then supplies a brief but lucid account of the fundamental tenets of the three principal systems of Indian philosophy, the Nyāya, the Sankhya, and the Vedanta, which may be roughly characterised as the Theistical, the Atheistical, and the Pantheistic systems. He next proceeds, in five books—headed respectively, (1.) A Partial Exposition of Christian Doctrine; (2.) The Evidences of Christianity; (3.) Natural Theology; (4.) Of the Mysterious Points of Christianity; and (5.) The Analogy of Religion to the Constitution and Course of Nature—to argue the truth of Christianity as a Divine revelation, grounded on miracles and prophecy, and supported by analogy; and to deduce from its truth, so established, the falsity of any of the dogmas of Indian philosophy, atheistic or pantheistic, which are inconsistent with its fundamental principles. It is the author's opinion that reason, left to itself, would conduct the inquirer to Pantheism, as the most probable theory of Universal Being; and he is therefore compelled to ground his refutation of that system on its inconsistency with the theistic principles inculcated by the Christian revelation, whose truth he has first sought to demonstrate. Though, however, this is the principal process applied by the author for the refutation of Hindu errors, he also urges the difficulties suggested against Pantheism by consciousness and reason; and argues against the necessity for admitting the practical inferences drawn by the Hindu teachers from their Pantheistic philosophy, even though the principles of that philosophy were admitted, or at least left *in dubio.*

We shall not attempt to discuss the momentous question here raised by the author, viz., Whether or not philosophical speculation necessarily issues in Pantheism; and whether, if it did so, we could have any basis left from which we could establish the truth of Christianity. This opinion is common to our author with the clergy of France, who of late years have been urging against the Spiritualist philosophers of that country, that there is no middle point between the Church and Pantheism. The maintainers of this view have lately been answered by a distinguished French metaphysician, M. Émile Saisset, who endeavours to prove the reverse of Dr Ballantyne's proposition, and to show that, on the contrary, philosophical speculation leads to a belief in one supreme, personal, intelligent, and righteous Creator and Governor of the universe.

Dr Ballantyne begins his preface by professing that his work is "but an imperfect sketch of what the writer would wish to offer as a help to the missionary among the learned Hindus. Many topics which might advantageously receive full treatment are here scarcely more than indicated." This is a correct account of his book, when compared with that complete discussion of the subject which we ought to have. It is evidently nothing more than an outline of the subject, and a brief specimen of what the author is capable of producing; and we trust that he may be able to carry out what he proposes, viz., to continue his work, and to expand this imperfect sketch into a full and exhaustive discussion of the important subject-matter.

The volume concludes with an Appendix of notes and dissertations, more or less closely connected with its central subject, which will all amply repay perusal.

Popular Tables, arranged in a new form, giving information at sight for ascertaining, according to the Carlisle Table of Mortality, the value of Lifehold, Leasehold, and Church Property, Renewal Fines, &c., the Public Funds, Annual Average Price and Interest on Consols from 1731 *to* 1858*; also, various interesting and useful Tables, equally adapted to the office and the library table.* By CHARLES M. WILLICH, Actuary and Secretary to the University Life Assurance Society. Fourth Edition. 1859. Pp. 192. London: Longman, Brown, Green, and Longmans.

In size little more than that of a pocket volume, Mr Willich presents to the public a fourth edition of his "Popular Tables," which, in forms admitting of easy reference, contain just that kind of information which every buyer and seller of property and invester of money ought to possess. It furnishes one of the most striking examples with which we are acquainted of the scientific condensation of facts gathered from a large induction. Statistical results are so skilfully combined and arranged as to make the solution of the most intricate questions a matter of simple reference. A careful perusal of the Tables produces a feeling of surprise at their variety and comprehensiveness. Few business men have not felt the want of such a book as this; and it cannot fail to be welcomed as a most important addition to the class of literature to which it belongs. Referring to the Index, it may be noticed that the book contains Simple and Compound Interest Tables; Investment Tables, with and without the contingency of human life; Annuity and Life Interest Tables, where one, two, and three lives are involved; Tables of the Values of Reversions and Presentations of Livings; Tables for Estimating the Fines to be paid on renewing Leases granted on one, two, or three lives, on the failure of one or more of the lives; Tables of Logarithms; Tables of Foreign Coins, Currency, and Measures; and many others of equal importance required in practice. We cannot, of course, give practical illustrations of all these Tables; but certain of them (Nos. IV., VI., VII., and XV.), published, we believe, for the first time, are so important and valuable, in connection with the practical working of the principle of accumulation, that we take the opportunity of pointing out their application and utility.

Suppose a person lending such a sum upon land as would form an annual charge of L.200 upon the rents for a period of 33 years, so as to repay principal and interest at the rate of 5 per cent.

The first and ordinary calculation gives the sum invested in such a transaction, ...	L.3200
But it is clear that, if the party making such an investment be so situated that he cannot reinvest the instalments of principal as they are paid up also at the rate of 5 per cent., the whole investment would not actually produce to him 5 per cent. over the whole period. Now, Table XV. tells us that, if no more than 3 per cent. be received on the instalments of principal after they are paid up, the sum that ought to have been paid for the annual rent-charge referred to, so as to secure 5 per cent. on the whole amount paid for the whole period, is only	2934
The difference being , ...	L.266

Comparatively small sums, like annuities and rents, can seldom be invested (except by large public companies) at so high a rate of interest as principal sums; and the Tables alluded to are thus of decided practical utility, and direct attention to one of the subtleties in transactions involving the element of future accumulation which is very apt to be overlooked.

Besides the Tables alluded to, Mr Willich's book contains many others pertaining to mathematical and philosophical subjects, which men of science will know how to appreciate.

We cordially recommend these "Popular Tables" to the acceptance and daily use of men of science and business, as combining simplicity and elegance of structure with the largest amount of practical utility. S. R.

PROCEEDINGS OF SOCIETIES.

British Association for the Advancement of Science. Meeting at Aberdeen, September 1859.

MATHEMATICAL AND PHYSICAL SCIENCE.

On the Necessity for incessant Recording, and for simultaneous Observations in different Localities, to investigate Atmospheric Electricity. By Professor W. Thomson.—The necessity for incessantly recording the electric condition of the amosphere was illustrated by reference to observations recently made by the author in the island of Arran, by which it appeared that even under a cloudless sky, without any sensible wind, the negative electrification of the surface of the earth, always found during severe weather, is constantly varying in degree. He had found it impossible, at any time, to leave the electrometer without losing remarkable features of the phenomenon. Beccaria, Professor of Natural Philosophy in the University of Turin a century ago, used to retire to Garzegna when his vacation commenced, and to make incessant observations on atmospheric electricity, night and day, sleeping in the room with his electrometer, in a lofty position, from which he could watch the sky all round, limited by the Alpine range on one side and the great plain of Piedmont on the other. Unless relays of observers can be got to follow his example, and to take advantage of the more accurate instruments supplied by advanced electric science, a self-recording apparatus must be applied to provide the data required for obtaining knowledge in this most interesting field of nature. The author pointed out certain simple and easily-executed modifications of working electrometers, which were on the table before him, to render them self-recording. He also explained a new collecting apparatus for atmospheric electricity, consisting of an insulated vessel of water, discharging its contents in a fine stream from a pointed tube. This stream carries away electricity as long as any exists on its surface, where it breaks into drops. The immediate object of this arrangement is to maintain the whole insulated conductor, including the portion of the electrometer connected with it and the connecting wire, in the condition of no absolute charge; that is to say, with as much positive electricity on one side of a neutral line as of negative on the other. Hence the position of the discharging nozzle must be such that the point where the stream breaks into drops is in what would be the neutral line of the conductor, if first perfectly discharged under temporary cover, and then exposed in its permanent open position, in which it will become inductively electrified by the aërial electromotive force. If the insulation is maintained in perfection, the dropping will not be called on for any electrical effect, and sudden or slow atmospheric changes will all instantaneously and perfectly induce their corresponding variations in the conductor, and give their appropriate indications to the electrometer. The necessary imperfection of the actual insulation, which tends to bring the neutral line downwards or inwards, or the contrary effects of aërial convection, which, when the insulation is good, generally preponderate, and which in some conditions of the atmosphere, especially during heavy wind and rain, are often very large, are corrected by the tendency of the dropping to maintain the neutral line in the one definite position. The objects to be attained by simultaneous observations in different localities alluded to were:—(1.) To fix the constant for any observatory, by which its observations are reduced to absolute measure of electromotive force per foot of air; (2.) To investigate the distribution of electricity in the air itself (whether on visible clouds or in clear air) by a species of electrical trigonometry, of which the general principles were slightly indicated. A portable electrometer, adapted for balloon and mountain observations, with a burning match, regulated by a spring so as to give a cone of fire in the open air, in a definite position with reference to the instrument, was exhibited. It is easily carried, with or without the aid of a shoulder-strap, and can be used by the observer standing up, and simply holding the entire apparatus in his hands, without a stand or rest of any kind. Its indications distinguish positive from negative, and are reducible to absolute measure on the spot. The author gave the result of a determination which he had made, with the assistance of M. Joule, on the Links, a piece of level ground near the sea, beside the city of Aberdeen, about 8 A.M. on the preceding day (September 14), under a cloudless sky, and with a light north-west wind blowing, with the insulating stand of the collecting part of the apparatus buried in the ground, and the electrometer removed to a distance of five or six yards and connected by a fine wire with the collecting conductor. The height of the match was three feet above the ground, and the observer at the electrometer lay on the ground to render the electrical influence of his own body on the match insensible. The result showed a difference of potentials between the earth (negative) and the air (positive) at the match equal to that of 115 elements of Daniel's battery, and therefore, at that time and place, the aërial electromotive force per foot amounted to that of 38 Daniel's cells.

Report on Changes of Deviation of the Compass on Board Iron Ships by "Heeling," with Experiments on Board the City of Baltimore, Aphrodite, Simla, and Sleeve Donard. By Mr JOHN T. TOWSON.—The author explained the manner in which the Compass Committee was first formed, in accordance with the advice of the Section he was then addressing, and that two reports had been drawn up, which, with the advice of the Astronomer Royal, had been printed and "presented to both Houses of Parliament by command of her Majesty." There were matters of consideration which the Compass Committee deemed incomplete; the one was the change which took place in iron ships in proceeding to the opposite hemisphere; the other, the change that was produced by what is technically denominated *heeling*, that is, when the deck of a vessel leaned over through the action of the wind or otherwise; if when looking towards the bow it slanted downwards to the right, it is said to heel starboard, if to the left, to heel port. The first question was undertaken by the late respected Rev. Dr Scoresby, who proceeded to Australia in the Royal Charter, and whose exertions in the pursuit of this branch of the inquiry shortened a most valuable life. The second question was the subject of his (Mr Towson's) present report. Having described the principles on which his graphic illustration was constructed, he pointed out the unexpected amount of deviation which this source of disturbance (heeling) brought about, amounting, in most instances, when the ship's head was in the position to produce the maximum effect, to two or three points in the standard compass, and often to a greater amount as far as the steering compass is concerned. He remarked on several particulars connected with this investigation. Generally the north end of the compass was drawn to the upper side of the ship,—the case with seven out of nine compasses on board the City of Baltimore, but in the two steering compasses the needles were drawn in a contrary direction. Mr Towson explained the theory on which this disturbance arose, partly from subpolar magnetism below the compass, and partly from the disturbance of the inductive magnetism of the ships. In such ships as those under consideration, the following empirical rule held good with respect to compasses favourably placed. When the vertical force as determined either by vibrating experiments or torsion on board the ship, maintained the ratio, as compared with the vertical force on shore, in the proportion of nine to fourteen, little or no effect was produced by heeling; and in the case of the Simla this plan of predicting the amount of error was adopted; a moveable upright magnet was applied so as to produce the before-named vertical force, when it was found, "with magnet in," no error was produced, although "with magnet out" it amounted to 24° from changing a heel of 10° starboard to 10° port. Another remarkable result appears to exist. He believed that when a ship was built with her head south-east or south-west, little or no effect would be produced by heeling. When examining the magnetic condition of the Sleeve Donard, they were surprised to find that the vertical was very nearly that which would give no effect from heeling. Their talented stipendiary Secretary (to whom is due the credit of drawing up the two reports already published) immediately suggested that her head could not have been east when building, which we had taken for granted; and on inquiry we found that on account of her great length she had been built diagonally, with her head south-east nearly.

On the Rapidity of Signalling through long Submarine Telegraphs. By Mr F. JENKINS.—This paper detailed certain experiments undertaken at the establishment of Messrs. R. S. Newall & Co., Birkenhead, with a view to verify the theory of retardation, and to supply certain constants required. This theory has been well developed by Professor Thomson, and is confirmed by the results of these experiments, which have indeed only been rendered possible by the peculiar construction of Professor Thomson's marine galvanometer. In this instrument momentum and inertia are almost wholly avoided by the use of a needle weighing only 1½ grain, combined with a mirror reflecting a ray of light which indicates deflexions with great accuracy. By these means a gradually increasing or decreasing current is at each instant indicated at its due strength: thus, when this galvanometer is placed as the receiving instrument at the end of a long submarine cable, the movement of the spot of light, consequent on the completion of a circuit through the battery cable and earth, can be so observed as to furnish a curve representing very accurately the arrival of an electric current. Lines representing successive signals at various speeds can also be obtained, and by means of a metronome, dots, dashes, successive A's, &c., can be sent with nearly perfect regularity by an ordinary Morse key, and the corresponding changes in the current at the receiving end of the cable accurately observed. The strength of the battery employed was found to have no influence on the results; curves given by batteries of different strengths could be made to coincide by simply drawing them to scales proportionate to the strengths of the two currents. It was also found that the same curve represented the gradual increase of intensity due to the arrival of a current, and the gradual decrease due to the ceasing of that current. The curves of arrival obtained for lengths of from 1000 to more than 2000 nauts, were found to agree very closely in general appearance with those given by Professor Thomson's theory (*Proceedings of the Royal Society*, May, 1855). In the curves representing dots and dashes sent at high speeds, successive dashes appear in quite a different part of the scale from that occupied by dots. It is in these cases obvious that no delicacy of relay will enable us to indicate both of these signals at a constant adjustment, nor does any increasing strength of battery help us,—for though the variations of intensity are absolutely increased, the relative position of such changes to one another on the scale remains unaltered. The magnitude of the first appearance of a current at the far end of a cable may, however, be increased by the use of powerful batteries, and delicate instruments would permit the faintest appearance to be observed. By these means one isolated signal might be sent with great rapidity. Returning to the consideration of successive signals, when the speed of transmission is diminished, the oscillations of the spot increase in size, those for dots and dashes overlap one another, and would give legible Morse signals by means of a relay. The amplitudes of oscillation representing any letter or letters were found to be proportional to the amplitude representing dots. The speed of signalling possible can therefore be measured by that amplitude as soon as in one case it is determined what speed of dot signalling is compatible with the reception of all other combinations of dots, dashes, and spaces. This amplitude is modified by the nature of the receiving instrument, by the nature of the signal, by the skill of the manipulator, &c. The possible speed of signalling was found to be very nearly proportional to the squares of the lengths spoken through; thus, a speed which gave 15 dots per minute in a length of 2191 nauts, reproduced all the effects given by a speed of 30 dots in a length of 1500 nauts. At these speeds, with ordinary Morse signals, speaking would be barely possible. In the Red Sea, a speed of from seven to eight words per minute was obtained in a length of 750 nauts. This result agrees very closely with the deduction from the experiments at Birkenhead, and apparently shows that the influence of electro-magnetic induction, due to the disposition of the cable in coils, does not very materially retard the possible speed of signalling. The amplitudes of oscillation representing dots can be thrown into a curve which will be the same for all lengths. By this curve we can determine from one single observation, on any cable, the amplitude of oscillation due to any speed, and, consequently, the possible speed of signalling on that cable. This method, however, of determining the possible speed of signalling, presupposes that a considerable length of the cable shall have been manufactured. Mechanical senders, and attention to the proportion of the various contacts, would materially increase the speed at which signals of any kind could be transmitted. The best trained hand cannot equal the accuracy of mechanism, and the slightest irregularity causes the current to rise or fall quite beyond the limits required for distinct signals. No important difference was observed between signals sent by alternate reverse currents, and those sent by the more usual method. The amplitude of oscillation, and consequent distinctness of signalling, was quite the same in the two cases. An advantage in the first signals sent is, however, obtained by the use of Messrs Siemens & Halshe's submarine key, by which the cable is put to earth immediately on signalling being interrupted, and the wire thus kept at a potential half way between the potentials of the poles of two counteracting batteries employed, and the first signals become legible, which, with the ordinary key, would be employed in charging the wire.

Remarks on the Discharge of a Coiled Electric Cable. By Professor W. Thomson.—Mr Jenkins had communicated to the author during last February, March, and April, a number of experimental results regarding currents through several different electric cables coiled in the factory of Messrs R. S. Newall & Co., at Birkenhead. Among these results were some in which a key connected with one end of a cable of which the other end was kept connected with the earth, was removed from a battery by which a current had been kept flowing through the cable, and instantly pressed to contact with one end of the coil of a tangent galvanometer, of which the other end was kept connected with the earth. The author remarked that the deflexions recorded in these experiments were in the contrary direction to that which the true discharge of the cable would give, and at his request, Mr Jenkins repeated the experiments, watching carefully for indications of reverse currents to those which had been previously noted. It was thus found that the first effect of pressing down the key was to give the galvanometer a deflexion in the direction corresponding to the true discharged current, and that this was quickly followed by a reverse deflexion generally greater in degree, which latter deflexion corresponded to a current in the same direction as that of the original flow through the cable. Professor Thomson explained this second current, or false discharge, as it has since been sometimes called, by attributing it to mutual electro-magnetic induction between different portions of the coil, and anticipated that no such reversal could ever be found in a submerged cable. The effect of this induction is to produce in those parts of the coil first influenced by the motion of the key a tendency for electricity to flow in the same direction as that of the decreasing current flowing on through the remoter parts of the coil. Thus, after the first violence of the back flow, through the key and galvanometer, the remote parts of the cable begin, by their electro-magnetic induction on the near parts, to draw electricity back from the earth through the galvanometer into the cable again, and the current is once more in one and the same direction throughout the cable.

On a New Species of Double Refraction. By Sir D. BREWSTER.—The author exhibited to the Section a number of beautiful double slips of glass, with small pieces of decomposed glass, which he had obtained from the Marquis Campana in Rome, interposed, which showed all the varied tints of Newton's thin plates, and then explained to the Section how, by the polarisation in two different places of the transmitted light and the interference of those which were retarded by internal reflexion at the surfaces of these very thin films, none of them the two-thousandth of an inch thick, the varied tints were produced. He also explained minutely their optical properties when examined by the polariscope.

On the Transmission of Electricity through Water. By Mr J. B. LINDSAY.—The author has been engaged in experimenting on the subject, and in lecturing on it in Edinburgh, Glasgow, and other places since 1831. He has succeeded in transmitting signals across the Tay and other sheets of water, by the aid of the water alone, as a means of joining the stations. His method is to immerse two large plates connected by wires at each side of the sheet of water, and as nearly opposite to each other as possible. The wire on the side from which the message is to be sent is to include the galvanic battery and the commutator or other apparatus giving the signal. The wire connecting the two plates at the receiving-station is to include an induction-coil or other apparatus for increasing the intensity and the recording apparatus. The distance between these plates he distinguished by the term "lateral distance." He found that there was always some fractional part of the power from the battery sent across the water. There were four elements on which he found the strength of the transmitted current to depend: first, the battery power; second, the extent of surface of the immersed metal sheets; third, the "lateral distance" of the immersed sheets; and, fourth, in an inverse proportion, the transverse distance or distance through the water. As far as his experiments led him to a conclusion, doubling any one of the former three doubled the distance of transmission. If, then, doubling all would increase the intensity of the transmitted current eight-fold, he entered into calculations to show that two stations in Britain, one in Cornwall and the other in Scotland, and corresponding stations well chosen in America, would enable us to transmit messages across the Atlantic.

On the Phonautograph, an Instrument for Registering Simple and Compound Sounds. By the Abbé Moigno.—The Phonautograph is an instrument which consists of a large chamber or drum, of a spheroidal form, with a diaphragm or drum-head at one end, which, by a system of levers, works the pen which records the sounds which the form of the chamber causes it to concentrate on the tympanum. The Abbé exhibited a drawing to the Section, which explained the construction of the instrument, and then exhibited drawings showing the actual markings of the pen over a sheet of paper carried past it by clockwork, first, when tuning-forks sounding various notes were vibrated in presence of the instrument; second, when several notes were sounded on a diapason pipe; and, third, when a person spoke before it. In the first two cases the recording pen drew such regular curves, that the number of vibrations corresponding to the note as seconds could be counted, and they were obviously the curve of sines. In the case of the human voice, the words spoken were written below the corresponding tracings of the pen; and although these were very irregular, yet a marked correspondence could be traced, especially where the words contained *r*'s, *g*'s, and other well-marked low or guttural sounds.

Report of the Balloon Committee. By Colonel Sykes.—The Report gave various preliminary details of the meetings and proceedings of the Committee; amongst these, that they secured the co-operation and use of the large balloon of Mr Green. That Professor Tyndal, and Mr J. B. Russell, and Mr John Murray, the two latter students in Glasgow University, who had been employed under Professor Thomson in charge of his meteorological instruments, had volunteered their services to accompany Mr Green, and to aid in making and recording the proposed observations. Colonel Sykes also informed the Committee that an observer of light weight was available from Greenwich, and also Mr Storks Eaton, an amateur meteorologist of Little Bredy, Dorset. The Committee selected Wolverhampton as the place of ascent, spring as the time, as suggested by the Astronomer Royal, and secured through Lord Wrottesley the use of the instruments which had been used in the former ascent. The Gas Company at Wolverhampton offered the use of their yard, from which the balloon might ascend, and in which it might be inflated. Various causes of delay occurred, but eventually M. Gassiot having reported the instruments and other arrangements all ready, Mr Storks Eaton was selected by the Committee to conduct the experiments, and at length General Sabine and M. Gassiot were invited to attend at Wolverhampton on Monday the 15th of August. On that day Colonel Sykes, Lord Wrottesley, Admiral Fitsroy, Dr Lee, and Mr Glashier attended at the place of ascent. In consequence of sudden, violent gusts of wind that day, Mr Green was unwilling to ascend, fearing damage to the valuable instruments; but as he declared that no damage to life was to be feared, he offered to risk the balloon if the Committee wished that the ascent should proceed. The Committee then ordered the gas to be laid on, but various delays having protracted the preparations to the approach of darkness, when the ascent would be unprofitable, it was deferred till next day. On that day, when all preparations were nearly completed, a sudden gust of wind jerked the funnel of the balloon, and caused such a rent as to render any attempt at an ascent on that occasion impossible. Mr Green assured the Committee it would take some weeks to repair the damage. Mr Green's terms were L.20 for the first ascent, L.15 for a second, L.20 for a third, and L.15 for a fourth, the Committee to provide the gas and pay all incidental expenses. The Committee offered to renew their operations early next year, and suggested that their reappointment should be recommended, and the grant of L.200 continued at their disposal, giving the opinion of Sir J. W. F. Herschel and other eminent scientific men that the objects to be attained were of the highest interest.

On the Focus of Object-Glasses. By Mr A. Claudet.—The researches on this question tended to show the relation between the distances and sizes of objects with focal distances and sizes of their images, and to find the two points, one before the lens and the other behind, from which the distance of objects and the focal distances must be measured, and from which all proportions are in an exact ratio; for it is found, that measuring from the object-glass on both sides, double distance of object does not produce one-half of the focal distance, and *vice versa.* These two points are, first, the point before the lens which produces an image infinitely larger at infinite distance, and behind the lens the point which is the focus for an object at infinite distance, giving an image infinitely small; it is obvious that these two points are on each side the zero of the scale of measure, and it remained to fix the position of another point before the lens, which produces behind the lens an image as large as nature. The two spaces between these two points, one in front and the other behind the lens, are perfectly equal, and they are each the unit by which all distances of objects and all focal distances are to be measured. Double the unit in front will give a focus one-half of the unit behind the lens, and one-half of the unit in front will give a focal distance double of the unit behind the lens, and all the other distances in the same proportion, so that knowing either the distance in front of the lens, or the focal distance, the other distance can be found without having to examine the focus on the ground-glass; the only thing to do being to divide the scale called "the unit of focal distances," in any number of parts corresponding in an inverted ratio with the progression of distances in front of the glass.

On a Changing Diaphragm for Double Achromatic Combinations. By Mr A. Claudet.—Mr Claudet explained the construction of his contrivance, intended to reduce or increase the aperture of a double achromatic lens without having to unscrew one of the lenses, and without any slit on the tube. This is done by two rings revolving on one another, like the top and bottom part of a snuff-box, and each carrying a number of india-rubber stripes, the other end of which was fixed on the opposite ring, so that making the ring not fixed in the tube to revolve by an external pinion, the india-rubber stripes were drawn intermingling with each other until each of them was extending on the diameter of the tube, on which disposition the whole aperture was shut. Mr Claudet exhibited also the very ingenious pupil diaphragm, invented by Mr Mauley, optician in Paris.

On a curious Landscape inclosed in a Specimen of Calcedony belonging to a Lady. Exhibited by Sir D. Brewster, and explained by him.—Sir D. Brewster, who had examined the specimen, ascertained that the landscape was not between two plates subsequently united, but was in the interior of a solid piece of calcedony. He stated that calcedony was porous, and that the landscape was drawn by a solution of nitrate of silver, which entered the pores of the mineral. Sir David also stated that, above thirty years ago, he had examined a similar specimen, belonging to the late Mr Gilbert Innes of Stow, who had paid a large price for it. Having no doubt that the figure of a cock, which it contained, was drawn by nitrate of silver, introduced into the pores of the mineral, he induced the late Mr Somerville, a lapidary in Edinburgh, to make the experiment, and he succeeded in introducing the figure of a dog into the interior of the mineral. "The curious fact, however, displayed by the specimen now exhibited to the Section is, that the landscape had entirely disappeared after being kept four years in the dark. When I received the specimen yesterday from Miss Campbell, the landscape was wholly obliterated; but after the exposure of an hour this morning, it reappeared in the distinctest manner, as may be seen by looking at it against a white ground." It is of importance to remark, that the figure of the cock in Mr Innes's specimen, which was very strong in its tint, has never been seen either to disappear or to diminish in its tints.

On the Present State and History of the Question respecting the Acceleration of the Moon's Motion. By the Astronomer Royal.—It had been known, from the time of Newton, that the motions of the moon are disturbed by the attraction of the sun, and that a great part of the effect is of the following kind, viz., that when the moon is between the sun and the earth, the sun attracts the moon away from the earth; and when the earth is between the sun and the moon, the sun attracts the earth away from the moon; and thus, in both cases, it tends to separate the earth and the moon, or diminishes the attraction of the moon to the earth. There are sometimes effects of the opposite character; but, on the whole, that just described is predominant. If this diminution were always the same in amount, the periodic time of the moon passing round the earth would always be the same. But it was found in the last century, by Halley and Dunthorne, that the periodic time is not always the same. In order to reconcile the eclipses of the moon recorded by Ptolemy with modern observations of the moon, it was necessary to suppose that in every successive century the moon moves a little quicker than in the preceding century, in a degree which is nearly represented by supposing that at each successive lunation the moon approaches nearer to the earth by *one inch*. The principal cause of this was discovered by Laplace. First, it had been shown by him and by others that the attractions of the other planets on the sun and on the earth do not alter the longer axis of the orbit which the earth describes round the sun, and do not alter the length of the year; but they diminish slowly, but continually through many thousands of years, the degree of ellipticity of the earth's orbit. Now, when the earth is nearest to the sun, the decrement of attraction of the moon to the earth (mentioned above) is greatest; and when the earth is furthest from the sun, that decrement is least. It had been supposed that the fluctuations of magnitude exactly balance. But Laplace showed that they do not; he showed that the increased amount of decrement (when the earth is nearest the sun) overbalances the diminished amount (when the earth is furthest from the sun); and therefore that the less eccentric is the earth's orbit, the less does the increased amount of decrement at one part overbalance the diminished amount at another part, and the less is the total amount of the sun's disturbing force. And, as the sun's disturbing force diminishes the moon's attraction to the earth, that attraction is less and less impaired every century, or becomes practically stronger; every century the moon is pulled into a rather smaller orbit, and revolves in a rather shorter period. On computing the effect from this cause, it was found to agree well with the effect which Halley and Dunthorne had discovered in observations. The lunar tables thus amended (and with other, but minor, improvements) were applied to the computation of other ancient eclipses which require far greater nicety than Ptolemy's lunar eclipses, namely, total eclipses of the sun. The most remarkable of these were the eclipse of Thales (which occurred at a battle), that at Larissa or Nimrud (which led to the capture of that city by the Persians from the Medes), and that of Agathocles (upon a fleet at sea). They are all of great importance in settling the chronology. Dates were thus found for these several eclipses, which are most satisfactory. About this time Mr Adams announced his discovery, that a part of the sun's disturbing force had been omitted by Laplace. The sun pulls the moon in the direction in which she is going (so as to accelerate her) in some parts of her orbit, and in the opposite direction (so as to retard her) in other parts. Laplace and others supposed that those accelerations and retardations exactly balance. Mr Adams gave reason for supposing that they do not balance. In this he was subsequently supported by M. Delaunay, a very eminent French mathematician, who, making his calculations in a different way, arrived at the very same figures. But he is opposed by Baron Plana, by the Count de Pontécoulant, and by Professor Hansen, who all maintain that Laplace's investigations are sensibly correct. And in this state the controversy stands at present. It is to be remarked, that observations can here give no assistance. The question is purely whether certain algebraical investigations are right or wrong. And it shows that what is commonly called "mathematical evidence" is not so certain as many persons imagine; and that it ultimately depends on moral evidence. The effect of Mr Adam's alteration is to diminish Laplace's change of periodic time by more than one-third part. The computations of the ancient eclipses are very sensibly affected by this. At present we can hardly say how much they are effected; possibly those of Larissa and Agathocles would not be very much disturbed; but it seems possible that the computed eclipse of Thales might be thrown so near to sunset as to be inapplicable to elucidation of the historic account. This is the most perplexing eclipse, because it does not appear that any other eclipse can possibly apply to the same history. The interest of this subject, it thus appears, is not confined to technical astronomy, but extends to other matters of very wide range. And the general question of the theory of the moon's acceleration may properly be indicated as the most important of the subjects of scientific controversy at the present time.

On the Establishment of Thermometric Stations on Mont Blanc. By Professor TYNDALL.—I proposed to the Royal Society some months ago, to establish a series of stations between the top and the bottom of Mont Blanc, and to place suitable thermometers at each of them. The Council of the Society thought it right to place a sum of money at my disposal for the purchase of instruments and the payment of guides; while I agreed to devote a portion of my vacation to the execution of the project. At Chamouni I had a number of wooden piles prepared, each of them shod with iron, to facilitate the driving of it into the snow. The one intended for the summit was 12 feet long and 3 inches square; the others, each 10 feet long, were intended for five stations between the top of the mountain and the bottom of the Glacier de Bossons. Each post was furnished with a small cross-piece, to which a horizontal minimum thermometer might be attached. Six-and-twenty porters were found necessary to carry all our apparatus to the Grands Mulets, whence fourteen of them were immediately sent back. The other twelve, with one exception, reached the summit, whence six of them were sent back. Six therefore remained. In addition to these we had three guides, Auguste Balmat being the principal one; these, with my friend Dr Frankland and myself, made up eleven persons in all. Though the main object of the Expedition was to plant the posts and fix the thermometers, I was very anxious to make some observations on the diathermancy of the lower strata of the atmosphere. I therefore arranged a series of observations with the Abbé Veullet, of Chamouni; he was to operate at Chamouni, while I observed at the summit. Our instruments were of the same kind; and in this way we hoped to determine the influence of the stratum of air interposed between the top and bottom of the mountain upon the solar radiation. Wishing to commence the observations at an early hour in the morning, I had a tent carried to the summit. It was ten feet in diameter, and into it the whole eleven of us were packed. The north wind blew rather fiercely over the summit; but we dropped down a few yards to leeward, and thus found shelter. Throughout the night we did not suffer at all from cold, though the adjacent snow was 15° Centigrade, or 27° Fahr. below the freezing point of water. We were all, however, indisposed. I was, indeed, unwell when I quitted Chamouni; but I fully expected to be able to cast this off during the ascent. In this, however, I was unsuccessful; my indisposition augmented during the entire period of the ascent. The wind increased in force towards morning; and as the fine snow was perfectly dry, it was driven upon us in clouds. Had no other obstacle existed, this alone would have been sufficient to render the observations on solar radiation impossible. We were therefore obliged to limit ourselves to the principal object of the expedition—the erection of the post for the thermometers. It was sunk six feet in the snow, while the remaining six feet were exposed to the air. A minimum thermometer was screwed firmly on to the cross-piece of the pile; a maximum thermometer was screwed on beneath this, and under this again a wet and dry bulb thermometer. Two minimum thermometers were also placed in the snow; one at a depth of six, and the other at a depth of four feet below the surface; these being intended to give us some information as to the depth to which the winter cold penetrates. At each of the other stations we placed a minimum thermometer in the ice or snow, and a maximum and a minimum in the air. The stations were as follows:—The summit, the Corridor, the Grand Plateau, the glacier near the Grands Mulets, and two additional ones between the Grands Mulets and the end of the Glacier de Bossons. We took up some rockets, to see whether the ascensional power or the combustion was affected by the rarity of the air. During the night, however, we were enveloped in a dense mist, which defeated our purpose. One rocket, however, was sent up, which appeared to penetrate the mist, and rising probably above it, its sparks were seen at Chamouni. Dr Frankland was also kind enough to undertake some experiments on combustion: six candles were chosen at Chamouni, and carefully weighed. All of them were permitted to burn for one hour at the top; and were again weighed when we returned to Chamouni. They were afterwards permitted to burn an hour below. Rejecting one candle, which gave a somewhat anomalous result, we found to our surprise, that the quantity consumed at the top was, within the limits of error, the same as that consumed at the bottom. This result surprised us all the more, inasmuch as the *light* of the candles appeared to be much feebler at the top than at the bottom of the mountain. The explosion of a pistol was sensibly weaker at the top than at a low level. The *shortness* of the sound was remarkable; but it bore no resemblance to the sound of a cracker, to which, in acoustic treatises, it is usually compared. It resembled more the sound produced by the explosion of a cork from a champagne-bottle, but it was much louder. The sunrise from the summit exceeded in magnificence anything that I had previously seen. The snows on one side of the mountain were of a pure light blue, being illuminated by the *reflected* light of the sky; the summit and the sunward face of the mountain, on the contrary, were red from the *transmitted* light, and the contrast of both was finer than I can describe. I may add, in conclusion, that the lowest temperature at the summit of the Jardin during last winter was 21° Cent. below zero. We vainly endeavoured to find a thermometer which had been placed upon the summit of Mont Blanc last year.

On Electrical Frequency. By Professor W. Thomson.—Beccaria found that a conductor insulated in the open air becomes charged sometimes with greater and sometimes with less rapidity, and he gave the name of "frequency" to express the atmospheric quality on which the rapidity of charging depends. It might seem natural to attribute this quality to electrification of the air itself round the conductor, or to electrified particles in the air impinging upon it; but the author gave reasons for believing that the observed effects are entirely due to particles flying away from the surface of the conductor, in consequence of the impact of *non-electrified* particles against it. He had shown in a previous communication (Section A, Thursday, Sept. 15), that when no electricity of separation (or, as it is more generally called, "frictional electricity," or "contact electricity,") is called into play, the tendency of particles continually flying off from a conductor is to destroy all electrification at the part of its surface from which they break away. Hence a conductor insulated in the open air, and exposed to mist or rain, with wind, will tend rapidly to the same electric potential as that of the air, beside that part of its surface from which there is the most frequent dropping, or flying away, of aqueous particles. The *rapid charging*, indicated by the electrometer under cover, after putting it for an instant in connection with the earth, is therefore, in reality, due to a *rapid discharging* of the exposed parts of the conductor. The author has been led to these views by remarking the extreme rapidity with which an electrometer, connected by a fine wire with a conductor insulated above the roof of his temporary electric observatory in the island of Arran, became charged, reaching its full indication in a few seconds, and sometimes in a fraction of a second, after being touched by the hand, during a gale of wind and rain. The conductor, a vertical cylinder about 10 inches long and 4 inches diameter, with its upper end flat and corner slightly rounded off, stood only 8 feet above the roof, or, in all, 20 feet above the ground, and was nearly surrounded by buildings rising to a higher level. Even with so moderate an exposure as this, sparks were frequently produced between an insulated and an uninsulated piece of metal, which may have been about $\frac{1}{10}$th of an inch apart, within the electrometer, and more than once a continuous line of fire was observed in the instrument during nearly a minute at a time, while rain was falling in torrents outside.

On Sir Christopher Wren's Cipher, containing Three Methods of finding the Longitude. By Sir D. Brewster.—Sir David said that at page 263 of his "Life of Sir Isaac Newton," the following paragraphs would be found:—"The bill which had been enacted for rewarding the discovery of the longitude seems to have stimulated the inventive powers of Sir Christopher Wren, then in his eighty-third year. He communicated the results of his study to the Royal Society, as indicated by the following curious document which I found among the manuscripts of Newton:—'Sir Christopher Wren's cipher, describing three instruments proper for discovering the longitude at sea, delivered to the Society November 30, 1714, by Mr Wren:—

'OZVCVAYINIXDNCVOCWEDCNMALNABECIRTEWNGRAMHHCCAW.

'ZEIYEINOIEBIVTXESCIOCPSDEDMNANHSEFPRPIWHDRAEHHXCIF.

'EZKAVEBIMOXRFCSLCEEDHWMGNNIVEOMREWWERROSHEPCIP.

'Vera copia. Edm. Halley.'

We presume that each of these paragraphs of letters is the description of a separate instrument. If it be true that every cipher can be deciphered, these mysterious paragraphs, which their author did not live to expound, may disclose something interesting to science." Sir David Brewster went on to say that soon after the publication of "The Life of Sir Isaac Newton," he had received a letter from Mr Francis Williams, of Grange Court, Chigwell, suggesting very modestly, that as the deciphering of the cipher, as published, was so simple, he supposed many persons had already done so; but if not, he begged to say that the mystery could be solved by reading the letters backwards in each of the three paragraphs, omitting every third letter. He had, on the approach of the Meeting of the British Association, received permission from Mr Williams to give an account to this Section of Mr Williams's method of solving the enigma. In his letter conveying the permission, which Sir David read, he suggests that "Sir Christopher Wren's object was to make it too mysterious to be of use to any one else. It is possible he may have wished to delay for a time the publication of his inventions, perhaps till he had improved his instruments, but was afraid that in the interval another would hit upon and publish the same discovery. He would send this cipher, then, to the Royal Society as a proof to be used at any future time." Sir David had the following explanation then, in accordance with Mr Williams's suggestion, written upon the black boards, the letters to be omitted being written in small characters to distinguish them, and backwards:—

WAcCHhMArGNwETrICeBAnLAmNCdEWcOUcNDxINiVAvCUzO.—Wach magnetic balance wound in vacuo (one letter a misprint). The omitted letters similarly read are—Chr. Wren, mdccxiv.

FIcXHhEArDHwIPrPEeSHnANmDEdSPcOIcSExTUiBEiONiEYiEZ.—Fix head hippes handes poise tube on eye (one letter a misprint). Omitted letters make—Chr. Wren, mdccxiiii.

PIcPEhSCrREwWErMOeVInNGmWHdEEcLScFRxOMiBEvAKzE.—Pipe screwe moving wheels from beake. Omitted letters make—Chr. Wren, mdccxiv.

The three last omitted z's occurring in the first part of each cipher to show that that part must be taken *last.*

On an Improvement in the Heliometer. By Mr N. Pogson.—The purpose of this communication is to suggest what I conceive to be a great addition to the power of any kind of micrometer used for measuring long distances on the double-image principle. It is therefore especially applicable to heliometers, and has indeed occurred to me chiefly from familiarity with the defects which have hitherto rendered this costly but magnificent instrument a comparative failure. It is well known to practical astronomers that the contact between two stars, however skilfully made, is a very unsatisfactory observation, even when the objects are pretty equal. But when one is a large bright star and the other a faint one, the difficulty and uncertainty amount to impossibility; for the faint star is invariably obliterated on approaching within two or three seconds of its superior. The alternative is then to diminish the aperture of that half of the object-glass through which the brighter star is viewed; but here again arises another evil; the disc is enlarged by diffraction, the value of the scale sensibly changed, and definition materially injured. Hence, parallax determinations of first magnitude stars, such as Arcturus and *a* Lyræ, cannot be satisfactorily made; but when the object is a double star, as, for instance, 61 Cygni or Castor, the comparison star can be brought *between* the components of the double star, and a most exquisitely perfect and comfortable measure obtained. Now, from having used the rock-crystal prism micrometer when residing at Oxford last year—then kindly lent me, together with a five-foot telescope of surpassing excellence, by Dr Lee—the idea occurred to me of introducing a prism, or achromatised wedge of rock crystal, into the heliometer, so as to double the image of the brighter star. By this means the dubious contact would be dispensed with; for the fainter object, by being brought midway between the two images of the bright star, would be precisely similar to the present easy observation of 61 Cygni, previously referred to. The prism could be of such a constant angle as to separate the two images to a convenient distance,—not too far, so as to render the estimation of distance difficult, but just wide enough to prevent the obliteration of a faint comparison star, before named as one of the evils to be avoided. The prism rather improves the appearance of a bright star than otherwise; and as the images are doubled, of course half the light of each is lost, equivalent to a considerable reduction of the aperture, thus obviating the third objection alluded to at starting. Armed with this addition to its strength, and taking the precaution never to observe on bad nights, when the atmosphere will not permit the use of powers from three hundred upwards—for I hold it as an absurdity to attempt to investigate tenths of a second of arc with anything less—the heliometer is doubtless yet destined to realise the highest expectations ever raised as to its efficiency for grappling with that most minutely intricate and vastly important research, viz., the parallax of the fixed stars.

On Chinese Astronomy. By Mr J. B. Lindsay.—The object of the present paper is to draw the attention of the Section to the fact, that much information may be derived from Chinese literature in order to perfect our astronomy. The "Chun-tsiu," written by Confucius, contains an account of thirty-six eclipses (several of them total), and several comets, falling stars, and meteorites. The first eclipse here recorded was in the year before our era 719, the last was in B.C. 494,—thus comprising 225 years. Confucius was born in B.C. 550, and died at the age of seventy-three in B.C. 477. In a book lately published I have given an extract of the thirty-six eclipses; but the whole of the "Chun-tsiu" deserves to be translated and published. I have myself made a translation of the whole *verbatim*, but should prefer seeing it published by another better acquainted with the Chinese. The "Chun-tsiu" is a short chronicle of events; but there is an extended commentary on it entitled the "Tso-chuen," by Tso-kin-ming, who was a contemporary and an intimate friend of Confucius. This work should, I think, be also translated, as it gives a detailed account of astronomical observations, and comes thirteen years further down than the work of Confucius. Another work, entitled the "Kwo-yu," supposed to have been by the same author, contains an Appendix by another person, bringing down the history to B.C. 453. The succeeding history was principally written, and the celestial phenomena recorded, by Szi-ma-tsien, who lived a century before our era. His work is entitled "Shi-ki," or Historic Memoirs. He was Imperial Historian, as was also his father,—and his work is extremely interesting, as giving an account not only of Chinese affairs, but also of the Scythians and Turks who were then on the north-west borders of China. The 123d chapter, recording foreign events, has been translated into French by Brosset, and is found in the *Journal Asiatique* for 1828. This chapter comprises the history of forty-three years, or from B.C. 140 to B.C. 97, shortly before the author's death. Small portions of the "Shi-ki" have been translated into English, but the whole deserves to be so. A translation of the whole Chinese history and literature before our era would not be voluminous; but the "Chun-tsiu," the "Tso-chuen," and the "Shi-ki" should, I think, be translated first. Extended notes would be necessary to render the whole intelligible, and the Astronomer Royal might append notes on the various eclipses. The ancient Chinese classics are nine in number,—five of the first class, and four of the second. The five of the first class are the "Shu-king," the "Shi-king," the "I-king," the "Li-ki," and the "Chun-tsiu." The "Shu-king" has been translated into French by Desguignes,—the "Shi-king" into Latin by Lacharme,—the "I-king" into Latin by Regis, and others,—the "Li-ki" into French by Callery; but the "Chun-tsiu" has not yet been translated into any European language. The four books of the second class have been often translated into Latin and French. Their names are, the "Ta-tteo," the "Chung-yung," the "Lun-yu," and "Mang-tszi," or Mencius,—scarcely any of which have been translated into English.

On the Decomposed Glass found at Nineveh and other places. By Sir D. Brewster.—He described the general appearance of glass in an extreme state of decomposition, when the decomposed part was so rotten as to break easily between the fingers, a piece of undecomposed glass being generally found in the middle of the plate. He then explained how, in other specimens, the decomposition took place around one, two, or more points, forming hemispherical cups, which exhibit the black cross and the tints of polarised light. In illustration of this decomposition, he showed to the Meeting three specimens, in one of which there was no colour, but which consisted of innumerable circular cavities with the black cross, these cavities giving it the appearance of ground-glass. In another specimen the film was specular and of great beauty, showing the complementary colours by reflection of transmitted light. In a third variety the films were filled with circular cavities exhibiting the most beautiful colours, both in common and polarised light.

On Mild Winters in the British Isles. By Professor **Hennessy.**—He pointed out the circumstance that the meteorological observations made during the late remarkably mild winter tended to confirm the law which he had already announced in a letter to General Sabine, which appears in the *Proceedings of the Royal Society* for 1858. This law is, that during mild winters the coast stations exhibit an increase of temperature more than inland stations, and that the temperature on the west and south coasts approaches towards uniformity. In France, as pointed out by M. Liais, the first part of this law is found to hold good, as evinced in the comparative climatology of Cherbourg and Paris. Mr Hennessy referred these phenomena to an abnormal extension of heat-bearing currents across the Atlantic. From the greater stability of such currents than those of the atmosphere, and from the important influence they undoubtedly exercise upon our climate, he is led to infer that we are rapidly approaching a period when it may become possible to foretell whether the winter shall be cold or warm by knowing the conditions of temperature and the movements of currents in the Gulf of Mexico and the Atlantic during the summer and autumn.

On the Inclination of the Planetary Orbits. By Mr J. P. **Hennessy.**—The author stated, that on consulting a synoptic table of the planetary elements, some law had been obtained for the other elements, but none hitherto for the inclinations of the several orbits. This he conceived arose from the inclinations being set down in reference to the plane of the earth's orbit; for he found that a very remarkable relation manifested itself when they were tabulated in reference to the plane of the Sun's equator. The author had written on the board two tables: one, the ordinary table in reference to the Ecliptic; the other, that to which he wished to draw attention, having reference to the plane of the Sun's equator. In the latter, it was seen as a general law, that the inclinations of the planetary orbits increased as the distances of the several planets from the Sun increased. Thus, the inclination of the orbit of Mercury to the plane of the Sun's equator was but 0° 19′ 51″, while that of Neptune was 9° 6′ 51″. The only considerable deviation from regular progression being found, as might be expected, among the Asteroids: of which, if we take Victoria as a type, her inclination is no less than 15° 42′ 15″. The author considered that the fact that the orbits of the larger planets, Jupiter, Saturn, Uranus, and Neptune, are not more inclined, would seem to confirm a surmise of Laplace, who, in his "Exposition du Systême du Monde," speculates on the order in which the planets were thrown off from the Sun, and supposes that Jupiter, Saturn, &c. were thus formed long before Mercury, Venus, the Earth, and Mars. If so, the oblateness of the Sun would in its condition at that time have tended more powerfully than in its subsequent or present state to keep the planets near the plane of its equator. The discovery of this law regulating the inclinations of the planetary orbits appeared to him another addition to the class of facts which establish the analogy between the solar system and that of Jupiter and his satellites, it being well known to astronomers that the inclination of the orbits of the latter to the plane of Jupiter's equator was a function of their distances and masses.

On the Dynamical Theory of Gases. By Professor **Clerk Maxwell.**—The phenomena of the expansion of gases by heat, and their compression by pressure, have been explained by Joule, Claussens, Herapath, &c., by the theory of their particles being in a state of rapid motion, the velocity depending on the temperature. These particles must not only strike against the sides of the vessel, but against each other, and the calculation of their motions is therefore complicated. The author has established the following results:—1. The velocities of the particles are not uniform, but vary so, that they deviate from the mean value by a law well known in the "method of least squares." 2. Two different sets of particles will distribute their velocity, so that their *vires vivæ* will be equal; and this leads to the chemical law, that the equivalents of gases are proportional to their specific gravities. 3. From Professor Stokes's experiments on friction in air, it appears that the distance travelled by a particle between consecutive collisions is about $\frac{1}{447000}$ of an inch, the mean velocity being about 1505 feet per second; and therefore each particle makes 8,077,200,000 collisions per second. 4. The laws of the diffusion of gases, as established by the Master of the Mint, are deduced from this theory, and the absolute rate of diffusion through an opening can be calculated. The author intends to apply his mathematical methods to the explanation on this hypothesis of the propagation of sound, and expects some light on the mysterious question of the absolute number of such particles in a given mass.

CHEMICAL SCIENCE.

New Process of Preserving Milk perfectly Pure in the Natural State, without any Chemical Agent. By the Abbé Moigno.—To preserve milk for an indefinite period is an important problem, which in France has been solved in three different modes. M. de Villeneuve was the first to preserve milk, solidifying it by the addition of certain solid ingredients, but it was no longer, properly speaking, milk. M. de Signac preserved it by evaporating the milk till it became of the consistence of syrup, rendering it a solid mixture of milk and sugar; still it could not be called milk. M. Maben also preserved it by excluding the air, and exposing it to an atmosphere of steam about 100° Cent.—thus depriving it of all the gases which it contained, and then hermetically sealing the filled bottles in which it had been heated. When about to leave for Aberdeen, I opened a bottle which had been closed by M. Maben on the 14th of February 1854; and after a lapse of five and a half years, I found it as fresh as it was the first day. M. de Pierre has greatly improved the discovery. The means which he employs to effect the preservation of milk is still heat, but heat applied in some peculiar way, by manual dexterity, first discovered by a Swiss shepherd. All that I am allowed to state is, that the effect of this new method of applying heat is to remove a sort of *diustore*, or animal ferment, which exists in milk in a very small quantity, and which is the real cause of its speedy decomposition. When this species of ferment is removed, milk can be preserved for an indefinite period of time in vessels not quite full, and consequently exposed to the contact of rarified air—a result which was not effected by the process of M. Maben, or rather that of M. Gay-Lussac, as they completely expelled those gases which otherwise would have rendered it sour. I have such full confidence in the success of M. de Pierre's process, that I had not the least hesitation in bringing along with me from Paris to Aberdeen a large vessel containing five gallons of milk, fearlessly trusting it to railroads and steam-boats, thus exposing it to all the incidents of the journey. I am so confident of the success of the process, that I pour out the contents of this large vessel into Scotch glasses, with the conviction that I am giving to the ladies and gentlemen of the British Association a milk as natural, as pure, and as rich as when it was taken from the cow in the fertile plains of Normandy. May this potion, so sweet and so pure, be a symbol of those sentiments of benevolent affection which France, flourishing and enlightened, entertains towards her noble and great sister England!

Owing to its greater specific lightness, cream ascends to the top of the vessel, but it can be easily made to diffuse itself through the milk by slightly shaking it before uncorking the bottle. As the vessel is not quite full, a small quantity of butter may have been formed, and the milk may have become somewhat less rich, but it will still be pure and natural milk, without any strange taste. Thanks to the progress of science, of which I am happy to be the representative, France can yield with profit to England her fruits, her vegetables, her eggs, and now offers her prepared milk for the wants of the army and navy, having nothing to fear from the longest voyages, nor from the excesses of heat and cold.

On a Symmetrical Arrangement of Oxides and Salts on a Common Type. By Dr Lyon Playfair.—Salts, according to the present views, may be constituted of an oxide and an acid; of an electro-positive element and an electro-negative salt radical; or on the type of water, in which the hydrogen is sometimes replaced by an electro-positive element, sometimes by an electro-negative compound. The author adopted the whole series of metallic oxides as typical of salts, supposing that two equivalents of the metal were present in all the oxides except the magnetic oxide. He contended that neutral salts are not formed on the type of a basic oxide, such as water, but on that of a neutral oxide, such as peroxide of manganese or peroxide of hydrogen, of the general formula, $O_2(MM)O_2$. Two equivalents of the oxygen in this type may be replaced in a neutral salt by an anhydrous acid, so that the general formula of a neutral salt is either $O_2(MM)A_2$, or half that value, in which A represents any acid. The author showed that many facts supported the idea that an anhydrous acid could substitute oxygen directly, and *vice versâ*. Thus, carbonate of manganese heated in air becomes peroxide, oxygen substituting the acid; while peroxide of copper loses oxygen in air, and becomes a carbonate. Barytes heated in air absorbs oxygen, and becomes a peroxide; heated with sulphuric acid, it becomes a sulphate: both oxide and salt being formed on the same type. The author then proceeded to show that as there are varieties of oxides, so also there are varieties of salts, each constituted on an oxide type. Salts of suboxides represent the protoxides; subsalts, with two equivalents of an oxide and one of an acid, are formed on the type of sesquioxides; while those with three of a base and one of an acid, like phosphate of soda, are formed on the type of magnetic oxide of iron. The sesqui-salts, on this view, are on the type of manganic acid, $O_3(MM)A_3$ being like $O_3(MM)O_3$. The author then proceeded to show how various relations became apparent, if the oxygen in the oxides were arranged in the simplest form of an axis and equator around the metallic nucleus, according to a conventional system on a plane surface. The existence or deficiency of symmetry in the structure of a body becomes thus indicated. As a general conclusion, when there is an equal balance in the molecules of oxygen, or of electro-negative bodies playing its part, then rest or neutrality results; when the structure wants balance or symmetry, then activity is manifested—basicity when the electro-positive molecules predominate; acidity when the electro-negative are in excess. By writing *minus* points to show the want of symmetry, it is possible to indicate *à priori* whether an acid is monobasic, bibasic, or tribasic. In conclusion, the author referred to the oxides of nitrogen, chlorine, and carbon as illustrations of the importance of symmetry. Writing them all on four-volume formulæ, it is necessary to double them when the compound has an uneven number of molecules of oxygen; but the oxides of an even number do not require this duplication. Further, it was shown that the symmetrical oxides are neutral or only feebly acid in character in the case of the oxides of electro-negative elements. Thus hypochlorous, chlorous, and chloric acids are uneven, like nitrous and nitric acids; while binoxide of nitrogen and the peroxides of chlorine and nitrogen are neutral, from there being a balance in the molecules of oxygen. In like manner, oxalic acid, with an uneven number of atoms of oxygen, is more powerfully acid than carbonic acid, where the conditions for symmetry are more nearly satisfied.

To exhibit a Photograph of Fluorescent Substances. By Dr GLADSTONE.—It is well known, on the one hand, that the chemical action of light resides mainly in the most refrangible rays, and on the other hand, that these rays are altered in their refrangibility and effect on the visual organs by fluorescent substances. It occurred to the author that such substances would probably exert little photographic action. Hence he had made two drawings on sheets of white paper, one in an acid salt of quinine, the other in a very pale solution of chlorophyll, and had taken photographs of them. Although the drawing in quinine was quite undistinguishable from the white paper, and the chlorophyll drawing nearly so, when they were viewed in the same camera for adjusting the focus they were strongly marked on the photographic image by the little chemical action that had been exerted by them. The sheets of paper, and the drawings developed on the glass plate, were exhibited, showing that what theory had suggested as probable, was true in fact.

On a New Mode of Bread-making. By Dr ODLING.—By this process the carbonic acid is produced independently of, and superadded to, the flour, which consequently undergoes no modification whatever. The carbonic acid gas is stored in an ordinary gas-holder, and is pumped therefrom into a cylindrical vessel of water, whereby the water becomes charged with gas. This soda-water is mixed under pressure with the flour, and the resulting dough becomes vesicular on removing the pressure. It is then divided into loaves and baked. This process is so rapid that in an hour and a half from the first wetting of the flour, a sack of flour is made into two-pound loaves. The advantages of this new mode are—its cleanliness; from the beginning to the end of the operation, neither the flour nor the water is touched by the human feet; it conduces to the health of the work-people; it is a very rapid process; it is certain and uniform; and it prevents any deterioration of the flour, so that by this process you can use flour which would require alum in the ordinary process.

Report on Field Experiments on the Essential Manuring Constituents of Cultivated Crops. By Professor VOELCKER.—The field experiments, which extended over a period of four years, had special reference to the turnip-crops. Dr Voelcker described the plan upon which these experiments were undertaken, and mentioned the results which were obtained. Amongst other points of interest to the agriculturist, it may be noticed, as the result of four years' experience in the growth of turnips under particular conditions.—1. That fertilisers destitute of phosphoric acid do not increase the yield of this crop; 2. That phosphate of lime applied to the soil in the shape of soluble phosphate (super phosphate) increases this crop in an especial manner, and that the practical value of artificial manures for root crops chiefly depends on the relative amount of available phosphates which they contain. Thus it was shown that 3 cwt. of superphosphate per acre produced as large an increase of turnips as 15 tons of farm-yard manure; 3. That ammoniacal salts and nitrogenised constituents yielding ammonia on decomposition, have no beneficial effect upon turnips, but rather the reverse; 4. That ammoniacal salts applied alone do not promote, as maintained erroneously, the luxuriant development of leaves; but that they produce this effect to a certain extent when salts of ammonia are applied to the land in conjunction with the mineral constituents found in the ashes of turnips. The Report likewise states that numerous analyses of turnips have been made, from which it appears that the more nutritious and least ripened roots invariably contain less nitrogen than half-ripened roots, or turnips of low feeding qualities. In the latter, the proportion of nitrogen was found, in several instances, two to two-and-a-half times as high as in roots distinguished for their good feeding qualities. Similar experiments upon wheat showed that nitrogenised ammoniacal matters, which proved inefficacious in relation to turnips, increase the yield of corn and straw very materially, and that the increase of wheat was largest when the ammoniacal constituents were associated with mineral matters.

GEOLOGY.

On the Geological Structure of the Vicinity of Aberdeen, and the north-east of Scotland. By JAMES NICOL, Professor of Natural History, Aberdeen.—The author said—It has been thought that a short sketch of the geology of this locality might interest our visitors from the south. To illustrate this generally, I have had a large copy of that portion of my Geological Map of Scotland prepared. This, of course, does not give minute details, but still I have no hesitation in saying that it is more accurate than any other, as I have not only corrected it in many points myself, but have had the use of much material collected by my friend Mr A. Cruickshank.

Though scarcely needed, it may be mentioned that Scotland consists of three natural geological divisions:—

1*st*. Southern region of Lower Silurian Rocks of Murchison or Cambrian of Sedgwick. This region consists of greywacke and clay slate, rising into lofty broad-backed mountains, separated by wide valleys—the dales of the old Borderers.

2*d*. Central region of Old Red, Coal, and Trap. This contains only about one-sixth of the surface (5000 square miles), but full two-thirds of the population of Scotland, and a far larger proportion of the mineral wealth and manufactures of the kingdom.

3*d*. Northern Region of Primary or Crystalline Strata, broken through by Granite, and set in a framework of newer formations. It contains two-thirds of the surface, but little more than one-fourth of the population. It is in this region we are now met, and to one portion of it that I mean specially to direct your attention.

The kernel of this whole region is the GRANITE. This forms some of the highest mountains, and some of the lowest land in the district; of the former I may mention Ben Macdhui (only rivalled in Britain by Ben Nevis) and the Cairngorum mountains on the north of the Dee; and on the south of that river Loch-na-gar, Mount Keen, Mount Battock, and other giants of the Southern Grampians. These, the principal mountains, are usually round, massive, dome-like, with a deep corry on one side, as if formed by the falling in of one-third of the mountain, and thus bounded by lofty, rudely prismatic precipices, rising from a dark, black tarn in the centre of the hollow. In consequence of decomposition, the granite mountains are usually covered with huge feather-bed-like rocks, piled up in cairns of rude masonry, and the shelter of the red deer and ptarmigan. The rock in these mountains is rather fine ground, uniform in structure, and often reddish coloured. It contains cavities in which the rock crystal or Cairngorum stone, the topaz, and the beryl are found. Bennachie, one of the outposts of these mountains on the north-east, though not high and easily accessible, is very interesting. It looks out on the south-west to the loftier ranges of the Grampians, with patches of snow even at the end of summer, and on the north-east over the plains of Buchan—low, undulating, and treeless, but rapidly changing, under the industry of the inhabitants, from bleak moors to fertile corn-fields.

A large portion of these north-eastern plains too consists of granite; in them, however, occupying the lowest, not the highest position, as in the mountains. This fact shows that the granite is the basis on which the strata rest, and hence is exposed where they have been cut away by denudation. A fine section of the granite is seen in the sea-cliffs south from Peterhead, where it is intersected by long, narrow gullies and deep caves, in which the restless surge of the North Sea keeps up an incessant tumult. Hence some of the more remarkable of these excavations have got the name of the Bullers of Buchan.

The rock, in this region, is red or gray, according to the colour of the felspar. It often contains hornblende, or is a syenite, as in the tract to the north of Huntly, and in other places again becomes almost a fine grained greenstone or diorite. This diversity of mineral character proves that the granite is not all of one period of formation. The veins of granite, in the granite itself, show this even more clearly. These are beautifully seen in Rubislaw quarry, close to the town, where there is one very remarkable vein of coarse granite, composed of very large twin crystals of orthoclase felspar and mica, in a basis of quartz, along with long broken prisms of schorl, Davidsonite, or impure beryl and garnets. The quartz in this vein is also remarkable for numerous cavities enclosing fluids, which Sorby uses as natural thermometers to tell the temperature or pressure under which the rock was formed. The latter, he says, was for the fine granite or main body of the rock 78,000 feet, for the coarse granite or veins 42,000 feet.

Of the stratified rocks the first, GNEISS, covers a wide extent in Aberdeenshire, and generally in close proximity to, or resting on, the granite. It is thus seen in the valley of the Dee above Braemar, reposing on the granite in thin even beds, at a low angle, and apparently undisturbed by the inferior igneous rock. In many parts of the low country the same relation occurs, the gneiss often forming the hills, the granite the intervening valleys. But in other cases, as in the hills north of Ballater, the two formations are seen side by side. The gneiss, in many localities, is full of granite veins; but whether these belong to the great mass of granite, or are of a different age, is not easily determined; and the question seems never to have been fully or fairly worked out. Such veins are well seen on the coast to the south of the city, especially near Girdleness and the Cove, and also in many parts of the mountain chain on the south side of the Dee. Veins of felspar, porphyry, and of trap, are known in the gneiss on the same coast, and in many other localities.

The gneiss is usually the common variety of quartz, felspar, and mica. But varieties with hornblende, passing into hornblende slate, are also common. The latter are well seen in the hills along Glen Muic, and up to the top of Morven. The beds of gneiss are seldom flat or even, more often highly contorted.

In the Braemar district the gneiss is covered by beds of limestone and quartzite—the latter, perhaps, only a variety of the gneiss. It often contains much magnetite, apparently replacing the mica. Indeed, iron, both as the oxides and pyrites, is very common in all these rocks; strongly impregnating many of the springs, and finding its way into the sands of the rivers and of the sea-shore. From the Cairngorum mountains great ridges of quartzite run north into Banffshire, and to the coast near Cullen. In some places in this region it appears to lie below the mica slate, but their exact relation is obscure. In other parts of the low country, as in Mormond Hill, the quartzite rests on the gneiss.

Mica-slate in Scotland is most common in the south-west Grampians; but in this district it becomes greatly attenuated to a very narrow zone. In the Glensheeard Stonehaven sections, the mica-slate appears to lie below the gneiss, and not over it, as usually represented. There are great tracts of mica-slate also in the north-west, between the Spey and Deveron, where it is intermixed with gneiss and clay-slate, but the relations of the deposits are little understood. It often contains garnets, more rarely andalusite, and some other minerals.

Clay-slate also covers a considerable share in this district, chiefly to the south of Banff and the Troup Head. It is quarried in several places for roofing-slates, as near the Troup Head, in the Foudland Hills, and near Gartly. These slates are wrought on lines of cleavage, the bedding being in general scarcely perceptible. It has been said that fossils—graptolites—occur in this rock; but there is no foundation for this statement. I formerly described these clay-slates as probably Silurian; but this is only a theory, and as the clay-slate in the southern Grampians appears to dip north below the mica-slate, this view now requires confirmation. In Glenshee a curious series of black carbonaceous slates, containing graphite, like those of Easdale, occur. Graphite is also found in other parts of this region, in the metamorphic strata—a most important fact in reference to the theory of these rocks.

The Old Red Sandstone chiefly occurs on the outskirts of the region we are considering. The principal mass within it runs south from Gamrie—a locality well known for its nodules with fossil fish. Another isolated, but interesting portion, occurs round the ancient Castle of Kildrummy, in which impressions of plants have been found. A curious mass of conglomerate at the Old Bridge of Don, probably belongs to the same deposit. On the southern limit of the moss, the Great Red Sandstone formation of Strathmore begins, and is well seen in huge beds of red sandstone and conglomerate near Dunnottar. The conglomerate must be regarded as marking rather the shore-lines or certain peculiar local conditions, than any particular zone in the formation.

At the other extremity of the moss, on the Spey, the Morayshire deposits begin, with numerous fishes at Pynet Burn, Dipple, &c. Still further west are the beds with reptilian remains at Elgin, probably in the upper Old Red, or some newer formation, but beyond the limits of this paper.

Higher deposits are only known in fragments. Such is the portion of lias near Turriff, perhaps *in situ;* but other masses of clay with lias fossils, as near Banff, are more probably drifted. So also the greensand and chalk-flints spread over the rising ground from Peterhead to Cruden—noticed and collected in 1834 by the late Dr Knight of this university—are apparently detrital masses. Their number, however, and state of preservation, show that strata of this age probably once existed here *in situ,* and perhaps they may still occur below the waters of the North Sea. I formerly noticed the analogy of these deposits to those in the south of Sweden, where lias rests on gneiss, and is covered by chalk; but Flamborough Head is the nearest point where the chalk is now known *in situ.*

Last of all are the great detrital masses of the drift or boulder-clay. This forms two very marked divisions, evidently formed under very opposite conditions of the land. 1*st*, The lower boulder-clay, composed of thick beds of firm, brown or gray clay, full of large striated stones, some of them several feet or yards in diameter, and evidently deposited in an arctic sea, round the shores of an ice-clad land rapidly sinking in the waters. Glaciers, as the striæ they have left on the rocks testify, must then have covered our mountains, and floating icebergs filled our ocean. Above this deposit are, 2*d*, Loose, distinctly stratified, sands and gravels, with rounded water-worn stones. These are clearly a portion of the lower masses reconstructed, as the land now freed from ice rose gradually above the waters. The brick-clays, some blue, some red, are again only the finer materials washed out in this process, and laid down in gulfs and bays, and the quieter parts of the sea along the coast. They contain Arctic shells—showing that the climate was still cold; and also, at Clayhills, in the very city, star-fish (*Ophiura*), bones of fish like the cod or haddock, and full 30 feet below the present surface, bones of a small duck. They are well seen at Belhelvie, Old Aberdeen, and Torrie, but occur in many other localities. All along the south coast, too, the fishermen dredge up, attached to the large mussel by its byssus, valves of the *Pecten islandicus* and the small *Leda oblonga*, shown by their colour to have been imbedded in similar red clays.

In the peat bogs we have remains of even a more recent period, but little anterior to our own. In them are found skulls with gigantic horns, and huge bones of the old urns. Two fine specimens of these skulls in the Museum—one from Belhelvie, another from Caithness—show the wide range of this noble species in former times. And here the proper geologic history of the district ends.

The Prince Consort having entered the Section Room, Sir C. **Lyell** rose and said:—No subject has lately excited more curiosity and general interest, among geologists and the public, than the question of the antiquity of the human race; whether or no we have sufficient evidence to prove the former co-existence of Man with certain extinct mammalia, in caves or in the superficial deposits commonly called drift or "diluvium." For the last quarter of a century, the occasional occurrence, in various parts of Europe, of the bones of man or the works of his hands, in cave-breccias and stalactites, associated with the remains of the extinct hyena, bear, elephant, or rhinoceros, has given rise to a suspicion that the date of man must be carried further back than we had heretofore imagined. On the other hand, extreme reluctance was naturally felt on the part of scientific reasoners to admit the validity of such evidence, seeing that so many caves have been inhabited by a succession of tenants, and have been selected by man, as a place not only of domicile, but of sepulture, while some caves have also served as the channels through which the waters of flooded rivers have flowed, so that the remains of living beings which have peopled the district at more than one era may have subsequently been mingled in such caverns, and confounded together in one and the same deposit. The facts, however, recently brought to light during the systematic investigation, as reported on by Falconer, of the Brixham Cave, must, I think, have prepared you to admit that scepticism in regard to the cave-evidence in favour of the antiquity of man had previously been pushed to an extreme. To escape from what I now consider was a legitimate deduction from the facts already accumulated, we were obliged to resort to hypotheses requiring great changes in the relative levels and drainage of valleys, and, in short, the whole physical geography of the respective regions where the caves are situated—changes that would alone imply a remote antiquity for the human fossil remains, and make it probable that man was old enough to have co-existed, at least, with the Siberian mammoth. But, in the course of the last fifteen years, another class of proofs have been advanced, in France, in confirmation of man's antiquity, into two of which I have personally examined in the course of the present summer, and to which I shall now briefly advert. First, so long ago as the year 1844, M. Aymard, an eminent palæontologist and antiquary, published an account of the discovery, in the volcanic district of Central France, of portions of two human skeletons (the skulls, teeth, and bones), imbedded in a volcanic breccia, found in the mountain of Denise, in the environs of Le Puy en

Velay, a breccia anterior in date to one, at least, of the latest eruptions of that volcanic mountain. On the opposite side of the same hill, the remains of a large number of mammalia, most of them of extinct species, have been detected in tufaceous strata, believed, and I think correctly, to be of the same age. The authenticity of the human fossils was from the first disputed by several geologists, but admitted by the majority of those who visited Le Puy, and saw with their own eyes the original specimen now in the museum of that town. Among others, M. Pictet, so well known to you by his excellent work on Palæontology, declared after his visit to the spot his adhesion to the opinions previously expressed by Aymard. My friend, Mr Scrope, in the second edition of his "Volcanoes of Central France," lately published, also adopted the same conclusion, although, after accompanying me this year to Le Puy, he has seen reason to modify his views. The result of our joint examination,—a result which, I believe, essentially coincides with that arrived at by MM. Hébert and Lartet, names well known to Science, who have also this year gone into this inquiry on the spot,—may thus be stated. We are by no means prepared to maintain that the specimen in the museum at Le Puy (which unfortunately was never seen *in situ* by any scientific observer) is a fabrication. On the contrary, we incline to believe that the human fossils in this and some other specimens from the same hill, were really imbedded by natural causes in their present matrix. But the rock in which they are entombed consists of two parts, one of which is a compact, and for the most part thinly laminated stone, into which none of the human bones penetrate; the other, containing the bones, is a lighter and much more porous stone, without lamination, to which we could find nothing similar in the mountain of Denise, although both M. Hébert and I made several excavations on the alleged site of the fossils. M. Hébert therefore suggested to me that this more porous stone, which resembles in colour and mineral composition, though not in structure, parts of the genuine old breccia of Denise, may be made up of the older rock broken up and afterwards redeposited, or as the French say *remané*, and, therefore, of much newer date, an hypothesis which well deserves consideration; but I feel that we are, at present, so ignorant of the precise circumstances and position under which these celebrated human fossils were found, that I ought not to waste time in speculating on their probable mode of interment, but simply state that, in my opinion, they afford no demonstration of Man having witnessed the last volcanic eruptions of Central France. The skulls, according to the judgment of the most competent osteologists who have yet seen them, do not seem to depart in a marked manner from the modern European or Caucasian type, and the human bones are in a fresher state than those of the *Elephas meridionalis* and other quadrupeds found in any breccia of Denise, which can be referred to the period even of the latest volcanic eruptions. But, while I have thus failed to obtain satisfactory evidence in favour of the remote origin assigned to the human fossils of Le Puy, I am fully prepared to corroborate the conclusions which have been recently laid before the Royal Society by Mr Prestwich, in regard to the age of the flint implements associated in undisturbed gravel, in the north of France, with the bones of elephants, at Abbeville and Amiens. These were first noticed at Abbeville, and their true geological position assigned to them by M. Boucher de Perthes, in 1846, in his "Antiquités Celtiques," while those of Amiens were afterwards described in 1855, by the late Dr Rigollot. For a clear statement of the facts, I may refer you to the abstract of Mr Prestwich's Memoir, in the Proceedings of the Royal Society for 1859, and have only to add that I have myself obtained abundance of Flint Implements (some of which are laid upon the table) during a short visit to Amiens and Abbeville. Two of the worked flints of Amiens were discovered in the gravel-pits of St Acheul—one at the depth of 10, and the other of 17 feet below the surface, at the time of my visit; and M. Georges Pouchet, of Rouen, author of a work on the Races of Man, who has since visited the spot, has extracted with his own hands one of these implements, as Messrs Prestwich and Flower had done before him. The stratified gravel resting immediately on the chalk in which these rudely fashioned instruments are buried, belongs to the post-pliocene period, all the freshwater and land shells which accompany them being of existing species. The great number of the fossil instruments which have been likened to hatchets, spear-heads, and wedges, is truly wonderful. More than a thousand of them have already been met with in the last ten years, in the valley of the Somme, in an area 15 miles in length. I infer that a tribe of savages, to whom the use of iron was unknown, made a long sojourn in this region; and I am reminded of a large Indian mound, which I saw in St Simond's Island, in Georgia—a mound 10 acres in area, and having an average height of 5 feet, chiefly composed of cast-away oyster-shells, throughout which arrow-heads, stone-axes, and Indian pottery are dispersed. If the neighbouring river, the Alatamha, or the sea which is at hand, should invade, sweep away, and stratify the contents of this mound, it might produce a very analogous accumulation of human implements, unmixed perhaps with human bones. Although the accompanying shells are of living species, I believe the antiquity of the Abbeville and Amiens flint instruments to be great indeed, if compared to the times of history or tradition. I consider the gravel to be of fluviatile origin; but I could detect nothing in the structure of its several parts indicating cataclysmal action, nothing that might not be due to such river-floods as we have witnessed in Scotland during the last half-century. It must have required a long period for the wearing down of the chalk which supplied the broken flints for the formation of so much gravel at various heights, sometimes 100 feet above the present level of the Somme, for the deposition of fine sediment including entire shells, both terrestrial and aquatic, and also for the denudation which the entire mass of stratified drift has undergone, portions having been swept away, so that what remains of it often terminates abruptly in old river-cliffs, besides being covered by a newer unstratified drift. To explain these changes, I should infer considerable oscillations in the level of the land in that part of France—slow movements of upheaval and subsidence, deranging but not wholly displacing the course of the ancient rivers. Lastly, the disappearance of the elephant, rhinoceros, and other genera of quadrupeds now foreign to Europe, implies, in like manner, a vast lapse of ages, separating the era in which the fossil implements were framed, and that of the invasion of Gaul by the Romans. Among the problems of high theoretical interest which the recent progress of Geology and Natural History has brought into notice, no one is more prominent, and, at the same time, more obscure, than that relating to the origin of species. On this difficult and mysterious subject a work will very shortly appear, by Mr Charles Darwin, the result of twenty years of observation and experiments in Zoology, Botany, and Geology, by which he has been led to the conclusion, that those powers of nature which give rise to races and permanent varieties in animals and plants are the same as those which, in much longer periods, produce species, and, in a still longer series of ages, give rise to differences of generic rank. He appears to me to have succeeded, by his investigations and reasonings, to have thrown a flood of light on many classes of phenomena connected with the affinities, geographical distribution, and geological succession of organic beings, for which no other hypothesis has been able, or has even attempted, to account.

Among the communications sent in to this Section, I have received from Dr Dawson of Montreal one confirming the discovery which he and I formerly announced, of a land shell, or pupa, in the coal formation of Nova Scotia. When we contemplate the vast series of formations intervening between the tertiary and carboniferous strata, all destitute of air-breathing Mollusca, at least of the terrestrial class, such a discovery affords an important illustration of the extreme defectiveness of our geological records. It has always appeared to me that the advocates of progressive development have too much overlooked the imperfection of these records, and that, consequently, a large part of the generalisations in which they have indulged in regard to the first appearance of the different classes of animals, especially of air-breathers, will have to be modified or abandoned. Nevertheless, that the doctrine of progressive development may contain in it the germs of a true theory, I am far from denying. The consideration of this question will come before you when the age of the White Sandstone of Elgin is discussed—a rock hitherto referred to the Old Red or Devonian formation, but now ascertained to contain several reptilian forms, of so high an organisation as to raise a doubt in the minds of many geologists whether so old a place in the series can correctly be assigned to it.

On the Chronology of the Trap Rocks of Scotland. By Mr A. GEIKIE. —The points to be proved were—first, that there is sufficient abundance of felspathic matter in the grits of the Silurian region of the Lammermoors to warrant the inference that felspathic matter was either ejected during the formation of these grits, or already in considerable abundance on the surface. Second, that the Silurians of the Lammermoors were contorted during the Upper Silurian period, probably between the upper and lower Ludlows, and that this contortion was attended with a wide-spread extravasation of felspathic matter. Third, that the Old Red Sandstone period was marked by powerful and long-continued volcanic activity, in several centres, as the Sidlaws, the Ochils, the Pentlands, and part of the hills of Lanark. Fourth, that the carboniferous period was characterised by the especial abundance and activity of volcanic centres,—so much so that there is not a well-defined zone of carboniferous beds which does not, at some part of the Lothians, display its intercalated sheets of ash or greenstone; but that these eruptions were marked by local centres alike in their extent and in the character of the erupted material. Fifth, that after the carboniferous series, there is a great gap in the chronology of the Scottish trap rocks, the next traces of subterranean movement being discernible in the lias of Skye; but that contemporaneous igneous rocks are not found until towards the top of the middle oolite, where among estuarine limestones and shales, there occur in Skye and adjacent islands enormous sheets of greenstone and basalt. Sixth, that as upper secondary rocks have still to be determined in the Hebrides, we have, at present, to pass from the oolitic traps of Skye to the basalts and ashes of Mull, which, as shown by their associated fossils, are tertiary, and, probably, miocene. Lastly, that the later basalts and ashes of Arthur's Seat ought, probably, to be referred to the later secondary or older tertiary period.

On the Origin of Cone-in-Cone Structure. By Mr H. C. SORBY.—This structure consists of an assemblage of imperfect cones, inclosing other cones, which all have their apexes in the same direction, and usually occur in bands parallel to the stratification of the work in which they are found. By examining their transparent slices with polarised light, the author has come to the conclusion that this structure is due to the growth of minute prismatic crystals, of more or less impure carbonate of lime, which, starting from particular points, grow upwards or downwards in such a manner that the peculiar and curious compound conical masses were formed by the interference of the crystals with each other, and with the uncrystallisable impurities of the rock.

Report on the Exploration of the Upper Silurians of Lesmahago, in terms of the Association's grant to Mr Slimon. By Mr D. Page.—During the last summer, Mr Slimon and his son had diligently explored the fossiliferous tract of Upper Silurian strata in the parish of Lesmahago, and the result of their operations had been to exhibit still further the highly fossiliferous character of the Nilberry Silurians, and to give ample indication of a very varied and curious crustacean Fauna, altogether new to Palæontology. Molluscous remains of well-known Upper Silurian genera had also been obtained in sufficient numbers to prove the affinities of the beds; and indications of both an aquatic and terrestrial Flora seemed by no means rare throughout the strata. The specimens obtained had a threefold value:—1st, As proving the true Upper Silurian epoch of the Nilberry strata, and thus affording a clue to the investigation of other Sub-Devonian tracts in Scotland, yet but very imperfectly understood; 2d, As adding new forms to the life of a former epoch, and thus extending the boundaries of our zoological knowledge; and, 3dly, As enabling the Government Palæontologists, who had recently published their first monograph on the Eurypteridæ, to understand more clearly the nature of this curious family of Crustaceans, and to correct what must now evidently appear as misinterpretations of their structure and affinities. In none of the beds explored, either now or during the whole of Mr Slimon's previous explorations, had there ever been detected any trace of fish-life. The report concluded with a recommendation, that a further grant of L.10 or L.20 should be given to Mr Slimon to continue his valuable explorations.

On certain Volcanic Rocks in Italy, which appear to have been subjected to Metamorphic Action. By Professor Daubeny.—Dr Daubeny called the attention of the Section to two products of volcanic action met with in Italy, the peculiarities of which, he thought, had not been fully explained. The first of these is the Piperino rock, met with so extensively about Albano, near Rome, which is distinguished from ordinary tuff not only by its greater compactness and porphyritic aspect, but likewise by the occurrence in it of numerous laminæ of mica, and crystals of augite, which tend to give it the appearance of a metamorphic rock, or of one which, although originally ejected as tuff, had been subsequently modified by the long-continued action of heat and pressure. The principal difficulty in the way of thus considering it arises from its alternation in several places with ordinary tuff, or with strata of loose scoriæ, as is well seen near Marino, so that it is difficult to conceive how the materials composing the Piperino could have been subjected to heat after their deposition in the form of tuff, without the intervening layers having been subjected to the same operation. The other volcanic product alluded to was the rock called Piperino, found near Naples, a brecciated material, in which wavy and nearly parallel streaks of a dark gray, brown, and often almost black, colour occur, impacted in a matrix which is for the most part ash-gray, and seems, mineralogically speaking, to resemble trachyte. The imbedded masses occur generally elongated in the same direction, as are also the pores which occur in the midst of the mass. These circumstances have been accounted for by supposing a stream of molten trachyte to have invaded a congeries of fragments of ordinary lava, and to have brought about their partial fusion; but the Piperino seems to constitute a part of the great tufaceous deposit which overspreads the neighbourhood of Naples, to which no such metamorphic action is ascribable, and that which has been lately met with in the new road now constructing above the suburb of the Chiaja at Naples lies imbedded in the midst of ordinary tuff. Dr Daubeny therefore conceives, that the peculiarities presented by both the rocks alluded to require further elucidation, and that their study might tend to throw some new light upon the effects of metamorphic action upon rocks in general.

On the Relations of the Gneiss, Red Sandstone, and Quartzite, in the North-West Highlands. By Professor NICOL.—Professor Nicol had visited the Highlands, and had arrived at a different conclusion as to the succession of certain crystalline and sub-crystalline rocks from that arrived at by Sir R. Murchison. He contended that the great series of rocks in question were of older date than that assigned to them by Sir R. Murchison, and endeavoured to prove, by a reference to the sections which he exhibited, that the order of super-position which he advocated was the correct one.

The President said, this question was a difficult one of interpretation, and the burden of proof lay upon those who, like Sir R. Murchison, contended that the highly crystalline rocks were of the newest date.—Sir R. Murchison, at considerable length, replied to Professor Nicol, referring to sections which he had prepared, and maintaining with great confidence that the order of super-position he had formerly contended for was the correct one. In company with Professor Ramsay, he had examined the country; and although they were aware of the difficulties of certain obscure sections here and there, he contended that, in no country he had ever examined, in any part of the world, had he ever seen a clearer order of super-position than that which he had endeavoured to point out—viz., the super-imposition of quartz rock upon the limestone.—Professor Ramsay confirmed the views of Sir R. Murchison, stating that he had noticed for miles the super-imposition of the quartz rock upon the limestone without any break, and felt not the slightest doubt upon the subject. Professor Sedgwick spoke in corroboration of the views of Sir R. Murchison and Professor Ramsay. Going hastily over the country, it certainly appeared to him that the order of super-position was that contended for by Sir Roderick, although it was perfectly possible that more extended observation might induce them to come ultimately to a different conclusion.

On the newly-discovered Reptilian Remains from the neighbourhood of Elgin. By Professor HUXLEY.—Having received specimens of sandstone containing what he considered traces of Reptilia, in order to work out the problem of their character he was put in communication with Mr Duff and the Rev. Mr Gordon, but for whose efficient co-operation his labours must have been in vain. He was fortunate to obtain specimens containing impressions which led him to conclude it was a reptile. and not a fish. He next obtained impressions in the sandstone of what appeared to have been once a bone, resembling the bony plates of an alligator, from which he came to the conclusion that the reptile was one of the crocodilian species. Looking for further coincidence, he had received a fossil, which Professor Agassiz had declared the most extraordinary he had ever seen; and a cast taken from it appeared to represent the tail of the old reptile. He then had a cast taken from a fossil having a most extraordinary cavity in it, which appeared to be its dorsal vertebræ; from another specimen he got a piece of vertebra, such as support the hips in crocodiles; and he, too, got a bit of sandstone having an impression of vertebræ, with marks peculiarly characteristic of the neck; and to ascertain what the teeth or head was like, they had obtained a piece of stone with the impression of an upper jaw and a series of teeth, essentially resembling those of a crocodile; and from these and other traces he came to the conclusion that it had been a crocodilian reptile allied to the Dinosaurian series, but presenting various points of difference from all existing or fossil species, and that the period of its existence must have been that presented by the green sandstone. He also gave an account of the impressions in other pieces of sandstone—which Mr Gordon had sent him—indicating another reptile, with curious palatal teeth, which, in honour of the Rev. Mr Gordon, he called *Hyperodapedon Gordoni*. He also received two bits of rock, one containing a reptilian impression like a staganolepis.

Professor Owen said no one could fail to be impressed with the extreme minuteness and accuracy with which Professor Huxley had examined the facts, and with the clearness with which the facts had been described; and still more with the accuracy and soundness of the deductions which Professor Huxley had made. The paper read afforded very instructive evidence of the value of the law of co-relation of structure; because, at the last meeting of the British Association at Leeds, he had arrived at the conclusion, from observing a portion of the bone then exhibited, that these specimens were reptilian in their nature, and had published that opinion in an article in the "Encyclopædia Britannica." He concurred entirely with the conclusions which Professor Huxley had drawn from a more complete view of those bones. He now for the first time began to feel that the evidence of the structure of the cranium was most interesting, and necessary to be made known before they had a complete and satisfactory idea of the nature of the staganolepis.

On Tertiary Fossils of India. By Mr W. H. Baily.—The object of this communication was to give merely a sketch of results from the study of a large suite of fossils, collected chiefly from Burmah and Tenasserim Province, by Professor T. Oldham, superintendent of the Geological Survey of India, the details being intended for publication in the "Memoirs of the Geological Survey of India." The majority of the fossils was stated to be of Eocene age, most of them having been obtained from the banks of the Irrawaddy and from Prome and its neighbourhood. Professor Oldham also collected Nummulitic fossils from Kurrachee Salt Range of the Punjab, Mammalian remains from the Sewalik group; fish teeth and scales from Heinlat, Tenasserim, and Carboniferous fossils also from Tenasserim Province. A list of the Tertiary fossils was given, the majority belonging to Mollusca and to the following other classes:—Articulata—Crustacea and Cirripedia; Radiata — Annelida and Echinodermata; Protozoa — Foraminifera. The collection was said to contain many new and undescribed species; and to present a facies of certain amount of resemblance generically, but not specifically, with those from the Tertiary deposits of Europe, whilst, on the contrary, it was mentioned as a somewhat remarkable fact, that the further we go back in geological time, so much the greater is seen to be the resemblance between the marine fossil Faunas of distant geographical areas; for instance, the Lower Silurian fossils of the furthest point yet reached in Arctic explorations are many of them absolutely identical with species from that formation found in our own country, whilst those from the more modern deposits of Cretaceous and Tertiary age continue their relations more by representation of forms than identity of species,—a fact confirmatory of the important observations made by the late Professor E. Forbes on the interesting subject of the distribution of species in geological time. Allusion was made to the various Memoirs on the Palæontology of India which have from time to time appeared, principally in the "Transactions and Proceedings of the Geological Society of London," by which we are made acquainted with the geological formation of a great part of that country, showing a succession of fossiliferous strata from the Upper Tertiaries, commencing with the mammalian remains of the Sewalik Hills, believed to be of Miocene age, and continuing through the Nummulitic group and other Eocene beds, the Cretaceous and Oolitic series, together with Lias and Trias, to the Carboniferous and Devonian or Upper Palæozoics.

On the Elephant Remains at Ilford. By Mr A. Brady.—The first fossil to which I wish to direct attention is the tusk of an enormous mammoth, which was discovered about two years since. It was lying on its side, about 14 feet below the present surface of the soil; and I had the honour of inviting Sir Charles Lyell, and other eminent geologists, to see it before it was disturbed. It belonged to an animal of the species *Elephas primogeneus*, and is identical with the Siberian mammoth, and, I believe, with the one found in Behring's Straits. The tusk was decayed at each end, the extremities being gone, but the part preserved was over 9 feet long, and of proportionate bulk. Some idea may be formed from this of the huge size of the animal of which it formerly formed a part. It was very much incurved, being so much bent back that the bone was not more than 4 feet 2 or 3 inches across in any part. Owing to the nature of the soil, the whole tusk was very friable, most of the gluten of the ivory being decayed, so that great care was required in moving it to prevent it falling to pieces. This was done in the usual manner by the authority of the British Museum, to whom, by permission of Mr Curtis, I presented the fossil; it was, however, I regret to say, much damaged by removal, notwithstanding the care bestowed. It was nearly a year afterwards before any more bones were found. I then obtained a large tibia, and two molar teeth, probably belonging to the same animal, as they were not a great way from the tusk. One of the latter was very large, weighing about 12 lb., though, from long use, much worn. From this, I infer that the mammoth to which it belonged must have been of great age. About the same time, I obtained several bones of a large rhinoceros. These, from their more compact nature, were less decayed; and the tibia and one side of the jaw were very perfect, several teeth being *in situ*. The other half of the jaw was smashed by the workman's pick before I saw it; but I saved several teeth. Like those of the mammoth, they were very much worn. Two of them I gave to the College of Surgeons. The rhinoceros has been referred to the genus Leptorhinus. Associated with these remains were some of the bones of a large ox, the horns and skull of which were very perfect, with several teeth *in situ*. There were also turned up, within the last month or two, some bones of a large ruminant, which I believe to be of the Minocero, or Irish elk; but I have not yet been able to get them exhibited. About thirty years since, the late Dr Buckland discovered the bones of a mammoth in this locality; and about the same time the late Mr Gibson obtained the beautiful collection of bones now in the Royal College of Surgeons. Associated with the remains of those giants of ancient days, are the skulls of Planorbis, Mico, Cyclon, Paludina, &c. And there are now living in the Roden, and other tributary brooks in the neighbourhood, the *lineal* descendants of these fossils, the ancestors of which enjoyed the same sunshine as the mammoth and rhinoceros, the aristocracy of those days. We boast not of the primary rocks of Scotland, but we have amongst us, living on the same estate as their ancestors, the humble Paludina, Planorbis, &c. They are interesting; for they form, as it were, the link between the past and the present order of things.

GEOGRAPHY AND ETHNOLOGY.

Notes on Japan. By Mr Laurence Oliphant.—The three ports of the empire visited by the Mission, and which fell more immediately under our observation, were Nagasaki, situated in the Island of Kinsin; Sowinda, a port opened by Commodore Perry on the Promontory of Idsa; and Yedo, the capital city of the empire. Of these, Nagasaki is the one with which we have been for the longest period familiar. In former times it was a fishing village situated in the Principality of Omura; it is now an imperial demesne, and the most flourishing port in the empire. It owes its origin to the establishment, at this advantageous point, of a Portuguese settlement in the year 1569, and its prosperity to the enlightened policy pursued by the Christian Prince of Omura, in whose territory it was situated; while its transference to the Crown was the result of political intrigues on the part of the Portuguese settlers, in consequence of which the celebrated Tageo Sama included it among the lands appertaining to the Crown. Situated almost at the westernmost extremity of the empire, at the head of a deep land-locked harbour, and in convenient proximity to some of the wealthiest and most productive principalities in the empire, Nagasaki possesses great local advantages, and will doubtless continue an important commercial emporium, even when the trade of the empire at large is more fully developed, and has found an outlet through other ports. The town is pleasantly situated on a belt of level ground which intervenes between the water and the swelling hills, forming an amphitheatre of great scenic beauty. Their slopes terraced with rice-fields; their valleys heavily timbered, and watered by gushing mountain streams; their projecting points crowned with temples or frowning with batteries; everywhere cottages buried in foliage reveal their existence by curling wreaths of blue smoke; in the creeks and inlets picturesque boats lie moored; sacred groves, approached by rock-cut steps, or pleasure-gardens tastefully laid out, enchant the eye. The whole aspect of Nature is such as cannot fail to produce a most favourable impression upon the mind of the stranger visiting Japan for the first time. The city itself contains a population of about 50,000, and consists of between eighty and ninety streets, running at right angles to each other—broad enough to admit of the passage of wheeled vehicles, were any to be seen in them—and kept scrupulously clean. A canal intersects the city, spanned by thirty-five bridges, of which fifteen are handsomely constructed of stone. The Dutch factory is placed upon a small fan-shaped island about 200 yards in length, and connected with the mainland by a bridge. Until recently, the members of the factory were confined exclusively to this limited area, and kept under a strict and rigid surveillance. The old *régime* is now, however, rapidly passing away; and the history of their imprisonment, of the indignities to which they were exposed, and the insults they suffered, has already become a matter of tradition. The port of Hiogo is situated in the Bay of Ohosaka, opposite to the celebrated city of that name, from which it is ten or twelve miles distant. The Japanese Government have expended vast sums in their engineering efforts to improve its once dangerous anchorage. A breakwater, which was erected at a prodigious expense, and which cost the lives of numbers of workmen, has proved sufficient for the object for which it was designed. There is a tradition that a superstition existed in connection with this dyke, to the effect that it would never be finished, unless an individual could be found sufficiently patriotic to suffer himself to be buried in it. A Japanese Curtius was not long in forthcoming, to whom a debt of gratitude will be due in all time to come, from every British ship that rides securely at her anchor behind the breakwater.

Hiogo has now become the port of Ohosaka and Miaco, and will, in all probability, be the principal port of European trade in the empire. The city is described as equal in size to Nagasaki. When Kæmpfer visited it, he found 300 junks at anchor in its bay. The Dutch describe Ohosaka as a more attractive resort even than Yedo. While this latter city may be regarded as the London of Japan, Ohosaka seems to be its Paris. Here are the most celebrated theatres, the most sumptuous tea-houses, the most extensive pleasure-gardens. It is the abode of luxury and wealth, the favourite resort of fashionable Japanese, who come here to spend their time in gaiety and pleasure. Ohosaka is one of the five imperial cities, and contains a vast population. It is situated on the left bank of the Jedogawa, a stream which rises in the Lake of Oity, situated a day and a half's journey in the interior. It is navigable for boats of large tonnage as far as Miaco, and is spanned by numerous handsome bridges. The port of Hiogo and city of Osaca will not be opened to Europeans until the 1st of January 1863. The foreign residents will then be allowed to explore the country in any direction, for a distance of twenty-five miles, except towards Miaco, or, as it is more properly called, Kioto. They will not be allowed to approach nearer than twenty-five miles to this far-famed city. Situated at the head of a bay, or rather gulf, so extensive that the opposite shores are not visible to each other, Yedo spreads itself on a continuous line of houses along its partially undulating, partially level margin, for a distance of about ten miles. Including suburbs, at its greatest width it is probably about seven miles across, but for a portion of the distance it narrows to a mere strip of houses. Any rough calculation of the population of so vast a city must necessarily be very vague and uncertain; but, after some experience of Chinese cities, two millions does not seem too high an estimate at which to place Yedo. In consequence of the great extent of the area occupied by the residences of the Princes, there are quarters of the town in which the inhabitants are very sparse. The citadel, or residence of the temporal Emperor, cannot be less than five or six miles in circumference, and yet it only contains about 40,000 souls. On the other hand, there are parts of the city in which the inhabitants seem almost as closely packed as they are in Chinese towns. The streets are broad and admirably drained; some of them are lined with peach and plum trees, and when these are in blossom must present a gay and lively appearance. Those which traverse the Princes' quarter are for the most part as quiet and deserted as aristocratic thoroughfares generally are. Those which pass through the commercial and manufacturing quarters are densely crowded with passengers on foot, in chairs, and on horseback, while occasionally, but not often, an ox-waggon rumbles and creaks along. The houses are only of two stories, sometimes built of freestone, sometimes sunburnt brick, and sometimes of wood; the roofs are either tiles or shingles. The shops are completely open to the street; some of these are very extensive, the show-rooms for the more expensive fabrics being up-stairs, as with us. The eastern part of the city is built upon a level plain, watered by the Toda Gawa, which flows through this section of the town, and supplies with water the large moats which surround the citadel. It is spanned by the Nipon; has a wooden bridge of enormous length, celebrated as the Hyde Park Corner of Japan, as from it all distances throughout the empire are measured. Towards the western quarter of the city, the country becomes more broken, swelling hills rise above the housetops richly clothed with foliage, from out the waving masses of which appear the upturned gables of a temple, or the many roofs of a pagoda. It will be some satisfaction to foreigners to know, that they are not to be excluded for ever from this most interesting city. By the Treaty concluded in it by Lord Elgin, on the 1st of January, British subjects shall be allowed to reside there in 1862; and it is not improbable that a great portion of the trade may ultimately be transferred to it from Ranagawa. There is plenty of water and a good anchorage at a distance of about a mile from the western suburb of Linagawa. The only other port which has been opened by the late Treaty in the Island of Nipon is the Port of Nee-e-gata, situated upon its western coast. As this port has never yet been visited by Europeans, it is stipulated that if it be found inconvenient as a harbour, another shall be substituted for it, to be opened on the 1st of January 1860."

On the Ethnology and Hieroglyphics of the Caledonians. By Colonel J. Forbes. Colonel Forbes developed his views in the following propositions:—1. Whether found singly or in groups, those circles not surrounding moot-hills or tumuli were erected for places of worship. They were also used as places for the administration of justice, and for the assembly of councils. 2. The number of stones in these fanes had reference to the number of individuals or families; and perhaps, in circles of greater proportions, were according to the number of towns or tribe, to be represented in the councils, or benefited by the sacrifices at any particular cromlech. Some of the cromlechs contained altars within the area. Occasionally the altars formed part of the inclosing circle, and in other cases the altars were outside of the circle. 4. In the same fane there were altars to more than one deity. 5. The origin of these fanes cannot be traced in any country; and nowhere, except in the Old Testament, does history or rational tradition fix the period when, or the people by whom, any one of these monuments was erected. 6. Open to the weather, incapable of being covered, and with long avenues of approach, the form of these fanes has apparently been devised in Eastern countries possessing a clear sky and warm climate. 7. These heathen fanes of Britain were afterwards used as places of Christian worship, but cattle continued to be sacrificed in them. 8. These fanes were also used as burying-grounds for Christians.

On the Arabic-speaking Population of the World. By Mr A. Ameuney (a Syrian).—The Arabic has twenty-nine letters, and, with the combinations and the vowels, make about thirty-six. Seven of these letters are, to a foreigner, exceedingly difficult to pronounce. The Arabic being an original language, it has, of course, the masculine and feminine genders—and the dual. It has more. It has a personal pronoun, and a pronoun attached to the verb, like the Latin *amo*. It has feminine in the singular and in the plural to the verbs—so, if two people happen to be in the next room, and they were talking, you would know whether they be ladies or gentlemen, or whether one be a lady or a gentleman; or whether the speaker be a lady or a gentleman, or whether the party spoken to be a lady or a gentleman. Not so in any other language—partly only in Greek. We have singular, dual, and plural—plural below No. 10, and above No. 10; we have a plural of plurals, and a collective plural and its plural. Let us see what we can do with these roots. Take the word *love*. We want to use it in English: we add *r*, and make lover, or *ing*, and make loving; or prefix *be*, and make beloved; but you have to say the place of love, the cause of love, and the course of love (they say it never runs smooth)! You have kill, and a knife, and butcher, and slaughter-house! We have nine letters, say a, b, c, and, by adding or prefixing one or more of these to the original, we make a word. One for the place, one for the instrument, one for the cause, and one for the passion. Take the word love, again, as a verb. You can only say might, should, or would, love; cause to love, command to love, ask to be loved, to be passionately in love, and to fall in love (which is the worst, I think). But with us, we have thirteen other letters, and, by prefixing or adding one or more to the original word, we change the meaning. We only change the accent of the noun, and make it a verb. You have something like it —a présent, and to presént, a récord, and to recórd. There are 65,000 words in the English Dictionary. We have 150,000 in the Arabic, and, when the derivatives are added, the language becomes really formidable. There are a few languages in which there is more than four or five names for an object. Yôu have sword, scimitar, and cutlass, but we have 150 names for this instrument of death. We have 160 for an old woman, 120 for the hyena, and I should feel ashamed to tell you how many for the lion, the camel, and the horse. It is all very well for a poet, who wants to rhyme his verses, to have many words at his command, but the language becomes very formidable for the scholar and the foreigner. The Arabs, who, of course, lived at first in Arabia, did not differ from other primitive nations. They traded with, warred against, hated, and loved their neighbours. Their wars were mostly with the Persians and the Abyssinians, for their poems refer to these nations in particular. They had their national assemblies, as we have here now. There was one in particular like the British Association—that is, comparing small with great things. During the month of Moharem they ceased their wars, and they met at Ackos, where the great poets recited their poems, and arbitrators decided which was the first, second, and third best. The first was then inscribed in letters of gold, and hung up at the Kaaba. We have seven of these poems (Moallakât), and many other lesser ones. Few nations have ever produced their equal—I speak not lightly of the poetry of other nations. It was my great desire to read Sir Walter Scott's poetry that urged me to learn the English language. They are passionately fond of their country. They have ideas equally as good as these lines,—

> Breathes there a man, &c.;

or,

> O! Caledonia, stern and wild.

I have read several of the best poets in English, French, Italian, and Latin, but all appear to me to write too much. An Arab poet says all he wishes to say in a few verses. I am sure all Arab poetry is burning with a strong passion. The nearest to it is Pope's "Eloisa and Abelard." The wars of Arabs have ever been either for women or horses, and their poetry is full of expressions about them. The eyes, the lips, the breath, the neck, and skin of a woman, have more names than I could tell you of. Terreack! breath of life; wine, coffee, water of life, and paradise. The Arabs in their native simplicity are frugal, can endure fatigue, hunger, and thirst; but the Arab can never become rich, because he is so generous. From the days of Abraham to this day, his great delight is to entertain strangers. They have no hotel charges. Brotherhood is one of their strong ties. One becomes a brother either by a present or service rendered. People who live in towns present—give to one of the chiefs, and he can travel amongst the tribes. Antar had made a war on a tribe, defeated it, and was leading the people into captivity. A man called out to him, "El Goman, Antar!"—that is, The Covenant. Antar asked him where and when he ever covenanted with him. I was, said the man, once at such a well watering my horse. You came and wanted to do the same, but your rope was too short. Bread and salt is another thing. The refuge another. Yet France wanted others to give up the refugees whom she turned out herself. Whether Christianity ever made any great progress among them we do not know. There are, however, many Christian tribes, especially in Hauran and Korak. But as soon as Mohammed appeared, the Arab mind took a different turn, and they became a conquering race. They, in fact, burst the bounds of their desert, and went out—

the Koran in one hand and the sword in the other—either submission or death. After a little while came the tribute, or redemption. People redeemed themselves by paying an annual tax, very small, and they lived in peace. Then they extended to Syria, Mesopotamia, Egypt, Tripoli, to the borders of the Alantire, &c. The Arabs are like the Anglo-Saxons. They conquer,—give their language, manners, and customs to the conquered nation,—and in a short time they make them Arabs.*

Botanical Society of Edinburgh.

Thursday, 10*th November*, 1859.—Mr ANDREW MURRAY, President, occupied the Chair, and delivered the following opening address :—

It is the custom for merchants once a year to make an inquiry into their affairs: to take stock, as they call it, balance their books, reckon their gains and their losses, and see what the progress of their business has been during the bypast year. It is a good and a salutary custom, and one which most of ourselves put in use to a greater or less degree in regard to our own past conduct and life, when the advent of another year invites our thoughts to such considerations. Let us, at the commencement of our New Session, follow this example; let us take a retrospective view of our position, and estimate the gains and losses which we as the representatives, at least as the only embodied representatives, of the Science of Botany in this city, have made during the past year. Let us see how the progress of Botany has been affected by the events of that period.

And first, let us, like stout men, look our losses in the face. These, gentlemen, have not been small. We have no loss in the actual progress of our science to deplore; we have no false step in the mode of conducting our investigations to anounce; we have no fundamental principle to correct. The science is firmly based on the natural system. Its principles are sound, and are being day by day worked out to more and more perfection. Our physiological views have received no rude check; and we are progressing steadily and surely in our search after truth, and in the acquisition of fresh knowledge. But if we have no loss in this respect to regret, we have suffered deep and heavy loss in the persons of some of those who have been mainly instrumental in bringing the science into this satisfactory position. We have to bewail the loss of Alexandre von Humboldt and of Robert Brown, the topmost trees of all the forest. You have already heard the eulogium and the history of their labours from the pen of our excellent Professor, and I shall not do myself the injustice, nor inflict on you the tedium, of retreading the same ground. You have also heard from him an account of the loss we have sustained in the death of Professor Agardh of Lund, the great algologist. Of our young friend and fellow-member, Dr Nichol, he has likewise spoken; and we have recorded in our minutes the sense we felt of his loss; but in speaking of him and of similar losses, I cannot refrain from uttering the reflection which has sometimes forced itself upon me, on the occasion of the death of some of those heroes of Science, who have long stood in the front rank, and full of years as well as honours, have at last succumbed before the assaults of time; that their removal was, perhaps, a less loss to Science than that of some promising young man just entering on his career. They are like old trees which stand out as great landmarks, but which have almost done growing. Their powers of work are nearly exhausted; while the fresh intellect, keen eye, and powers of labour of the young man, might justify us in anticipating a greater harvest from him were he spared than perhaps the giant could now produce. The loss of the great man is felt keenly by those who come in contact with him. His stores of information, applied by a profound and practised intellect, render his loss to them irreparable. To-day they might go to him secure of getting information on any subject they had on hand; to-morrow, not all the reading, nor all the correspondence with all the learned in Europe, could procure it. To them the loss is irreparable. But to you or to me, and the general scientific world, who had no access to his well-stored mind, and look for no new work from his pen, the loss is one of sentiment and feeling. In the pure practical, selfish point of view, the talented young man is probably the greater loss of the two. But in this practical and selfish light, a greater loss than either is that of the matured man, as yet in the full vigour of life, prolific in work, accomplished in science, and eager and zealous in its pursuit. And such a loss we have sustained since we last met. Arthur Henfrey, Professor of Botany in King's College, London (in which chair he succeeded Edward Forbes), has been taken from us in the prime of life and intellectual vigour, and in the full career of laborious and successful research. He was only thirty-nine years of age when he died on the 7th of September last—not a great space of time to acquire the position and to leave the numerous contributions to Science which he has done. His walk was specially vegetable physiology and histology. His contributions to the Royal and Linnean Societies of London were numerous and valuable; the last of which is a paper in this year's Proceedings of the Linnean Society, on the Morphology of the Balsaminaceæ. Not his last literary bequest, however; for he was in course of contributing papers on Vegetable Structure to "The Journal of the Royal Agricultural Society of London;" and the last proof-sheets of the second edition of his papers in the "Micrographic Dictionary" were only out of his hands a few days before his death.

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* The Report of the Proceedings of the British Association is taken partly from the "Athenæum," and partly from Authors' abstracts.

Such, gentlemen, are the losses by which the past botanical year has been distinguished. Let us now turn to the more cheerful side, and reckon up our gains. In doing so I shall pass lightly over the not light labours of descriptive and of systematic botanists. Hooker, Bentham, De Candolle, Lindley, Miers, Bennett, Anderson, Berkeley, with many others, are ably continuing their labours in this department of botany. But time will not permit me to do more than merely notice the fact, that much progress has been made in this most important and most necessary, although rather dry branch of our science. Neither shall I have to detain you with a long list of new botanical works. In that respect, the year has not been fruitful. I see a new German book ("Die Pflanzendecke der Erde von Ludwig Rudolph," Berlin, 1859), which attracted my notice from being a sort of anticipation of one which most of us know is in progress by Professor Balfour and Dr. Greville,—a Climatic Flora; showing the character impressed upon different countries by the vegetation peculiar to them. It is illustrated with a number of coarse woodcuts, representing the different regions referred to. It shows that the idea has got abroad, or has originated in other minds besides those of our friends; and suggests the desirableness of pushing on more rapidly the work to which we all look forward with so much pleasure.

But if little has been done this year in the way of producing botanical works of a general nature, a great deal has been done in the way of procuring materials for such works in the future. Much new material regarding the Flora of special regions has been published; many short notices of new plants have appeared; and various accounts of the results of public and private expeditions have either already been published, or shortly will be so. It will, I hope, not be uninteresting to the Society, if I give a hasty notice of what has been done in this way. But, first, I would direct your attention to the fact, and claim your mutual congratulations upon it, that by far the greater portion of this work has been done by members of this Society, and men educated in this school. In every one of the public expeditions which have been sent out by Government, the post of Naturalist is filled by one of our body. Of the expedition up the Niger, Dr Balfour Baikie is Chief. In the United Presbyterian Mission at Old Calabar, which, although not a public scientific expedition, has assumed so much scientific interest and importance in the eyes of the public as almost to be looked upon in that light, we have no less than three members of this Society zealously working for us—Mr Baillie, Dr Hewan, and the Rev. Mr Thomson. In Livingstone's Expedition to the Zambesi, we have Dr Kirk and Mr Bankes, who, I think, is also an alumnus of this school; and to Captain Palliser's Expedition to the Rocky Mountains, Dr Hector is Geologist and head of the Naturalist Department. And these are not our only members who are ably working for us abroad. I shall, as I go along, have occasion to refer to others.

I shall now, following the sun round the globe, give a short summary of the progress which has been made in our knowledge of Geographical Botany during the past year.

In Europe, few new discoveries have been made; and in Britain, I believe, none. There may be one or two additions made to our Flora by the discovery of some minute cryptogamic plants which have escaped my notice; but I have better authority than my own (my friend, Mr M'Nab's) for saying that nothing of any moment has been added to our Flora. In Africa, a great deal has been done, although little has yet been published. Mr Charles Barter, of the Niger Expedition, had addressed two letters (dated January and March 1859) to Sir William Hooker, giving an interesting account of the vegetation of tropical West Africa. These letters have been published in the Linnean Society's Proceedings of this year, but I do not find much that is new. He states that orchids were very scarce; aquatic plants, which might have been expected to be numerous, were not found to be so. His list only contains thirteen, among which is the *Papyrus antiquorum*. In regard to the economic expectations from the vegetation of Africa, he says, "Too much must not be expected of Central Africa as a cotton-producing country; the plant needs more moisture than it would obtain in much of the land of the interior, and water-carriage should never be far distant in a country where all loads are conveyed by canoe or on the heads of men and women. There is plenty of available land near the sea and by rivers; the great valley of the Niger would alone yield an enormous supply. It is here cotton must be looked for, and its growth encouraged. The great plains of the interior are almost as useless in this respect as Sahara itself."

During upwards of two years' exposure to the climate, Mr Barter enjoyed excellent health under the most peculiar and trying circumstances; and it is only recently that the news of his death has reached England, from a rapid attack of dysentery at Balba, and while surrounded with comparative comforts—the first death that has occurred (such has been the care and attention devoted to health) among Dr Baikie's small party. Mr Barter's place has been supplied by the appointment of Mr Gustave Mann, who is to sail for Lagos on 24th November.

Of the Old Calabar station, the fruits are only now beginning to come in,—zoology having taken precedence of botany in the interest of the missionaries. They are now making up for lost time, and both specimens in spirits and living plants in Wardian cases (as well as seeds) have been liberally sent home. The information received from these proves of much importance, and puts us right on some points in which we had fallen into error from imperfect materials. The entire history and structure of the poison-bean is now known, and will, I hope, be described by Professor Balfour either here or in the Royal Society this winter. There are other novelties to be described; and when the seeds and the contents of the Wardian cases develop themselves, there will be still more.

Dr Livingstone's expedition does not seem to have yet reported progress on botanical points. But passing on to India, a good deal has been done. Those of you who heard Dr Hunter's exposition to the Society last winter, and Dr Cleghorn's papers read, will readily admit that the economic department is in good hands. I also notice a valuable and interesting paper by another member of this Society, and alumnus of this school, in the Journal of the Asiatic Society of Bengal, No. II., 1859, entitled "Notes on the Flora of Lucknow, with Catalogues of the Cultivated and Indigenous Plants," by Thomas Anderson, M.D. This paper has especial value as regards the geographical distribution of plants in India, inasmuch as he has paid particular attention to distinguishing those plants which have been introduced from those which are wild. This has hitherto been greatly neglected. A botanist coming to India, and finding an Indian plant growing in any locality, is very apt to set it down as indigenous to the spot, although, in point of fact, it has been introduced from some other parts of India by the natives in these gardens, from which it has escaped through boundaries being broken down and the gardens abandoned, the luxuriant soil and climate of India being peculiarly favourable to such naturalisation. Dr Anderson, in eliminating the genuine wild plants of his district from those introduced or cultivated, has performed a most useful service to the Indian botanist, and his example will doubtless be followed in other parts of the Peninsula. Dr Lindley has also, in the Linnean Society Transactions, given a valuable contribution to the Orchidology of India.

I must not omit to record a work on the Mosses of India by Mr Mitten—a laborious work, but one labouring under the disadvantage of want of plates, an auxiliary almost essential in all minute natural history researches.

Much has been done during the past year in zoology in the Indian Archipelago; but an equal return has not been obtained by botany. Still, if she has had less interest in the scientific researches in that quarter, she has not had to mourn, like zoology, the murder of her votaries; and only grieves for the loss of Mr Motley on grounds common to all humanity.

Passing to Australia, we find that the labouring oar there has been taken by Dr Mueller, Government Botanist for the colony of Victoria. He has contributed several papers to the Linnean Society during the past year, the chief of which are contributions to the knowledge of the Acaciæ of New Holland, and a monograph of that curious tribe of plants the Eucalypti. Besides these, he has made a report to Government on the plants collected during Mr Babbage's expedition into the north-western interior of South Australia in 1858; and he published the first parts of a separate work on the plants of Australia.

Returning northward by New Zealand, I may notice a paper by Mr Ralph on the Tree-ferns of New Zealand. And proceeding onwards to China, we find Mr Bentham revising some parts of his Hong Kong Flora; but since Mr Fortune's last expedition, there has been an intermission in the receipt of botanical novelties from that quarter. Japan, however, makes up for China.

Japan is the country to which all eyes are now turned. This so long hermetically sealed kingdom is at last opened. The merchant is rushing to it with his commodities, doubtless to meet the usual fate of new markets—wealth *primis venientibus*—a glutted market, and ruinous depreciation to those who follow. The naturalist is preparing to follow—nay, has already tasted of the long-forbidden fruit. The Russian expedition which concluded the treaty betwixt Russia and Japan had with them a naturalist, M. Gashkevitch, who made considerable collections, a portion of which has been described and published in the Russian scientific journals, but the much larger portion lost in the shipwreck of the Russian frigate, Diana, consequent upon a terrible earthquake. Other collectors have been more fortunate, and the botany of that region has been so far explored as to allow Dr Asa Gray to give a most interesting and valuable report upon it in the "Memoirs of the American Academy of Arts and Sciences," New Series, vol. iv., and abridged in the last number of "Silliman's Journal." I call this paper most important, not only on account of the interest which attaches to Japan at present, but from the philosophic spirit in which he uses his materials, and the important inferences which he draws from them in regard to two most interesting questions now occupying the minds of men of science—the mode of distribution of species, and the question of the origin of species; and as I have come to a different opinion from him on more than one of these points, I shall take the liberty to point them out to you. From Dr Gray's observations, it appears that, notwithstanding the comparative proximity of Japan to Western North America, there are actually more of its species represented in far distant Europe than in that country; also,—showing that this difference is not owing to the separation by an ocean—that far more Japanese plants are represented in Eastern North America than in either. And if, instead of looking at representative species, we regard the identical species only in the several floras, the preponderance is equally against Western as compared with Eastern North America, but is more in favour of Europe; for the number of Japanese species given by Dr Gray as also found in Western North America is about 120; in Eastern America, 134; in Europe, 157.

In relation to this, Dr Gray further states that he had already pointed out, in his "Statistics of the Flora of the Northern United States," "1. That a large proportion of extra-European types found in America are shared with Eastern Asia; and 2d, That no small part of these are unknown in Western North America. But," he goes on, "Mr Bentham was first to state the natural conclusion from all these data—though I know not if he has ever yet published the remark—viz., that the interchange between the temperate floras even of the western part of the Old World and of the New has mainly taken place *via* Asia. Mr Bentham also calls to mind how frequently large American genera (such as Eupatorium, Aster, Solidago, Solanum, &c.) are represented in Eastern Asia by a small number of species, which gradually diminish, or altogether disappear, as we proceed westwards towards the Atlantic limits of Europe; whilst the types peculiar to the extreme west of Europe (excluding, of course, the Arctic flora) are wholly deficient in America. These are among the considerations which suggest an ancient continuity of territory between America and Asia, under a latitude, or at any rate with a climate more meridional than would be effected by a junction through the chains of the Aleutian and Kurile Islands."

So far Mr Bentham; but Dr Gray adds—"The deficiency in the temperate American flora of forms at all peculiar to Western Europe is almost complete, and is most strikingly in contrast with the large number of Eastern American forms repeated or represented in Eastern Asia." "Let it also be noted, that there are even fewer Western European types in the Pacific than in the Atlantic United States, notwithstanding the similarity of the climate."

Now, I pray you to observe, that in Mr Bentham's remarks he is not speaking of identical species—for it will not be denied that the number of identical species found in America, and also in Asia, is very exceptional, and in drawing general conclusions, it is always best, in the first instance, to put exceptional cases out of view—but he is speaking of representative or congenerous species; and although Dr Gray is perfectly alive to the difference between the argument drawn from an identical species and one drawn from a congenerous species, he draws the same inference from congenerous species that he would have done from identical. At one place he says—"The discovery of numerous closely related species, thus divided between two widely separated districts, might not, in the present state of our knowledge, suggest former continuity, migration, or exchange; but that of identical species, peculiar to the two, inevitably would." And yet not half a page distant he says—"That representation by allied species of genera, peculiar, or nearly peculiar, to two regions, furnishes evidence of similar nature, and of equal pertinency with representation by identical species, will hardly be doubted." Now this I utterly and wholly deny. I have satisfied myself, and I trust before I have done I shall be able to satisfy you, or at all events I shall be able to make you understand the grounds which satisfy me, that the distribution of identical species and the distribution of congenerous species are wholly different things, and not necessarily either subject to the same laws, nor necessarily furnishing evidence of similar nature and equal pertinency. My view is, that the identity of a species is proof that it has somehow found its way from one of the places where it is found to the other; but that the presence of a congenerous species is merely the evidence that similar conditions of life, at the period of their creation, prevailed at the spots where congenerous species were created. Given the same physical conditions in all respects at two different places, say Australia and Japan, I hold that the creative product will be typically the same (not the identical species, but the type will be the same). Of course this does not affect nor take from the value of the inference of *connection* to be drawn from the fact of two places having had similar physical conditions. On that argument Mr Bentham and Dr Gray may be right—probably are right; but I object to the inference being drawn from the existence of congenerous species in different places, as if that, *per se*, indicated anything. It is the cause of these congenerous existences which may be used as an argument, not the existences themselves. Such abstraction from the argument being made, I might legitimately argue that the very reverse of the course of immigration or distribution indicated by Dr Gray and Mr Bentham was the true one—that the connection was between America and Europe, and not between America and Asia; for Dr Gray tells us that the identical Japanese species which occur in Europe are 157; in Eastern America, 134; in Western America, 120. The natural inference from this, if no other facts come to derange our calculation, is, that these plants have come from Japan to Western America *via* Europe—that is, first to Asia, then to Europe, then to Eastern America, and last to Western America. I do not give any opinion as to this, however. I wish merely to endeavour to adjust correctly the principles on which the reasoning is to be conducted.

Another point on which I suspect I differ from Dr Gray is on the origin of species. He does not commit himself to them; but from the the terms in which he speaks of the views of Darwin and Wallace, I should incline to reckon him a supporter of them. It is an exceedingly interesting and important subject, and well worthy of our devoting a few minutes to see how it stands, which I the more readily do, because it gives me an opportunity of saying a word or two in explanation of the feelings which induced me to accept the honour of being your President, when your kindness put it within my power; for you are not to suppose that I have sat in this chair for the last twelve months without frequent qualms of conscience as to the propriety of so imperfect a botanist as I am occupying it, whilst so many so much better qualified in that respect sit around me. Gentlemen, I assure you I have not accepted it in the confidence of self-sufficient ignorance. I am perfectly aware of my deficiencies—much more so than you can be who know less of them—but it appeared to me that the science of Botany was so closely connected with the allied natural sciences, that it might tend to the advantage of both were the cords of attachment drawn nearer, by a President more familiar with one department being occasionally chosen to preside over the other. A similar course has at times been followed in our fellow Society, the Royal Physical; and I felt that, with such a Vice-President as Professor Balfour to support and prompt me, I might gratify my own feelings by accepting the honour, and occasionally, perhaps, be of use in bringing zoological facts to bear upon botanical questions. The present is a case in point, and, I think, well shows that, in considering any subject of philosophic moment relating to Botany, Zoology must not be left out of view. The position of the matter is this:—Last year, Sir Charles Lyell and Dr Hooker brought before the Linnean Society certain papers, which, by dint [of pressure, they had prevailed upon Mr Darwin to allow them so to use, containing his and Mr Alfred Wallace's views on the origin of species. These papers consisted of an uncompleted essay by Mr Darwin, not originally intended for publication; a letter from Mr Darwin to Dr Asa Gray, the gentleman of whom we are now speaking; and a paper on the same subject by Mr Alfred Wallace, who had, independently of Mr Darwin, come to similar conclusions. These were, that there was no limit to the varieties which might proceed from species; that the breeding of domestic animals had shown the extent to which this might be carried; that these varieties become permanent; and that, under favourable circumstances, the variety might deviate so far as to constitute a new species; that the mode in which such new species might be supposed to take its ground was not according to Lamarck's hypothesis, that the supposed wants or longing of an animal ended in producing the required or desired structure. To use Mr Wallace's words—"The powerful retractile talons of the falcons and the cat tribe have not been produced or increased by the volition of these animals; but among the different varieties which occurred in the earlier and less highly organised forms of these groups, *those always survived longest which had the greatest facilities for seizing their prey.* Neither did the giraffe acquire its long neck by desiring to reach the foliage of the more lofty shrubs, and constantly stretching its neck for this purpose, but because any varieties which occurred among its antitypes with a longer neck than usual *at once secured a fresh range of pasture over the same ground as their shorter-necked companions, and on the first scarcity of food were thereby enabled to outlive them.*" Wallace, p. 6.) With a limited number of very curious facts, which I have not space here to notice (but which all appear to me to be disconnected with the theory by the absence of some link), and with an unlimited amount of time, these gentlemen think they have succeeded in giving, in this way, a clue to the origin of species; and, of course, if it is once admitted as possible to a small degree, there is no reason why it may not be extended to the whole. The theory has excited much attention. It is unnecessary to say it is a most important one. Sir Charles Lyell has formally given in his adhesion to it, in a speech which he made at the meeting of the British Association in Aberdeen; and Mr Darwin has in the press a volume treating of the whole subject; for it will be observed that the hints thrown out in these Linnean Society Papers are very brief, and merely indicate their authors' views on one or two points of a great question, leaving the greater part untouched on. I happen to know, however, from a friend who has seen the proof-sheets, that the views entertained and arguments relied on by Mr Darwin in his book are the same as those in his paper, more expanded and better illustrated; and as my objections go to the root of his theory, I may, without waiting for the book, indicate one or two zoological facts which appear to me to be inconsistent with it. In the first place, the theory, if good for anything, must be universal. It must not be merely one of several ways, or one of two ways, of creating species; or, at all events, if it does not apply to genera (though where the line is to be drawn I cannot see), it must be the way which is followed in those cases of congenerous species to which we have been alluding. With them, at least, it must be the sole way, if it is a way at all; and, to say truth, Mr Darwin and Mr Wallace do not seem to shrink from this inference; for Mr Darwin says—"Each new variety or species, when formed, will generally take the place of and exterminate its less well-fitted parent. This I believe to be the origin of the classification and affinities of organic beings *at all times*." Assuming, then, the position to be, that this is *the* way in which species are created, or rather developed, if I can point to any instance where a congenerous species exists under circumstances where access to its allied species is morally impossible, I should say the theory must fall. In a paper on an allied subject, now in the press for next number of the "Edinburgh Philosophical Journal," I notice several of such instances. The most striking, and the one which, to my mind, at once disposes of the whole matter, is the existence of species of the same genera of eyeless insects, existing in the vast subterranean isolated caves of Carniola, allied, and exceedingly closely allied, to similar species in the caves of Hungary—to similar but different species in the caves of the Pyrenees—to similar but different species in the caves of Auvergne—and, more than all, to similar but different species of the same genera in the Mammoth Cave of Kentucky. Each of these sets of caves has a different set of species, of the same genera, and all very closely allied. The physical condition of the place being the same, the product has been the same; but not by immigration, nor any means of distribution which we can imagine, can identical species—(for, remember, the theory implies that congenerous species are identical species, or, what is the same thing, their descendants)—be found in caves so widely separated; and it is not the common case of congenerous species found very wide apart, which yet may have traversed the intervening space, because these insects are found nowhere but in the caves, and not in them until you have penetrated far, far into the interior, usually about a couple of miles. Another instance may be drawn from our own coast. We have a small beetle which lives here between high and low water-mark (*Æpus fulvescens)*, between the leaves of shale. A closely allied form, but quite distinct *(Thalasobius testaceus)* is found in like circumstances on the coast of Chili. Here, again, like physical condition, like product. Take another case—although, perhaps, scarcely so isolated as these two. Of late years, ants' nests have been found to contain a considerable number of species of beetles which live with the ants, are often excessively like them, and sometimes are unprovided with eyes. The same peculiarity prevails here—allied species, and nothing but allied species in ants' nests wherever they are. For instance, among the beetles so found is the curious genus *Paussus*. Seventy or eighty species of *Pauss* are now known, and all are inmates of ants' nests, and confined to them. Species have been found in these localities in Spain, in Natal, in Hong Kong, in India, in Australia, and so on. Here, again, like physical condition, like product. I might draw similar illustrations from the parasites in bees' nests and wasps' nests. Further, it were easy to draw abundance of proof of the fact that congenerous species are at all events always found in similar physical conditions of life. Dr Gray allows this, although he applies the fact differently from me. He says—"Whether or not susceptible of scientific explanation, it is certain that related species of phænogamous plants are commonly associated in the same regions, *or are found in comparatively approximate* (*however large*) *areas of similar climate.*" But I must not dwell longer on this subject. It is a very suggestive one, and readily leads one away to meet or consider objections or difficulties which will occur to any one who thinks over it; and I would only beg any of you, who may think they see a flaw in my theory, not to take it for granted that I have not an answer for it. Perhaps, when Mr Darwin's book appears, I may examine the matter more in detail in another paper, either here or elsewhere.

Another very interesting topic, closely related to this, is also discussed by Dr Gray in this valuable paper. It is the inquiry into the distribution of the ancient flora of the northern half of the globe, as connected with its present distribution. Starting with the fact, which is now pretty generally admitted, that the present vegetation is not of recent creation, he sketches, in a clear and plausible manner, the probable geological changes which have taken place since the Tertiary Period, tracing the variations or oscillations which the climate, and consequently the ancient flora, would sustain. But for this I must refer you to the paper itself. I do not know whether his views of the antiquity of our present vegetation are accepted to their full extent by botanists in general, I rather think they wait for further information. He seems to rest them chiefly on the investigations of Mr Lesquereux, who conceives that he has identified in the tertiaries and subsequent deposits many of our present trees. For instance, in the tertiaries of Vancouver's Island, he identifies the *Sequoia sempervirens*—a tree now found ten or fifteen degrees further south—and one which grows to as great a size as the *Wellingtonia gigantea.* But I must leave Dr Gray, and hurry on to a conclusion.

Crossing from Japan to North America, we have Capt. Palliser's British North American Exploring Expedition, to which, as already mentioned, our member (Dr Hector) is Geologist and Chief Naturalist. A French gentleman, M. Bourgeau, acts as Botanical collector to the expedition; and two letters, respectively of June and October 1858, from him to Sir W. Hooker, are published in last year's Linnean Society Proceedings. They are not without interest, but I do not find anything of particular novelty. The information, both as to the plants and country, quite corresponds with what we know of them from Jeffrey and other sources. A table of the temperature of the earth and of forest trees, made at Fort Saskatchewan, furnishes data which may be useful to the generaliser.

A vast number of new pines have been described, or rather, I should say, announced under names, during the last year or two. These are chiefly from Mexico; and although many may be new, I have no doubt that a still greater number will turn out to be mere varieties or synonyms. The Botanic Garden here is, I may observe, not only well supplied with the cones and leaves, &c. of the Californian and other pines, but also has an exceedingly good collection of fine, healthy growing specimens. These unfortunately, however, are confined to one or two small plots, which might, perhaps, hold two or three examples of the trees when they reach their full size, but of course are quite inadequate to contain anything like the crowd now packed in them. Space must be had for them; and I do hope that Parliament may, in another year, be concussed into doing something for this most urgent object. But it is obvious that this will only be done through force of concussion and external pressure. Members must remember that the Botanic Garden is not a matter alien to them. Next to the University, they may be said to be the parties who have the greatest interest in its prosperity; and I would remind them, that so long as they choose to continue members of this Society, it is their duty to exert themselves on behalf of the Garden. I scarcely think that the members of societies now-a-days sufficiently consider the obligation which they undertake by joining them. They get themselves proposed and are admitted, pay their fees, and think that that is all their part of the contract—in consideration of which they have the privileges of members, come to our meetings when they see any paper in the billet which takes their fancy, and in all other respects conduct themselves as if they were no more members of the Society than of one at the Antipodes. But I should wish much to get them to look at it in another light. I should like them to think that they are part of the Society, and that the Society is part of them; and that it is not optional with them, but a real matter of duty to push on, support, and sustain it by every means in their power. I am not so wild as to dream of introducing the laws of the Oineromathic—poor Edward Forbes's early chivalrous association—in which everybody was to help everybody, under every circumstances, in purse and person. But it is not utopian to expect members, who feel themselves qualified for it, to take the trouble of giving us occasional papers—and I know several in this Society who might well do it, and do not;—it is not utopian to expect one and all of us to exert ourselves to add to the stores of the Museum—although here, I must admit that this duty is more conscientiously performed; and lastly, and what specially led me to speak upon the subject, it is not too much to expect us, one and all, to clamour loudly and perseveringly, at every fitting season, and at every fitting place, for a more liberal support from Government to the Garden and its Officers.

Gentlemen, I think I have completed the hasty survey of the globe which I undertook, for all that South America has contributed during the last year need not detain us; and in concluding, as the term of my Presidency expires this evening, I have only now to thank you most cordially, in the first place, for having honoured me so highly as to place me in this chair; and in the next place, for the uniform kindness and forbearance which you have extended to me in my imperfect efforts worthily to fill it.

The following Communications were read:—

1. *Letter from* Dr John Kirk, *Physician and Naturalist to the Livingstone Expedition, relative to the Country near Lake Shirwa, in Africa.* (With a Plate.)

Senna, May 11, 1859.
On board Steam Launch.

My Dear Dr Balfour.—From our former letters you may have heard of the difficulty we found in ascending the Zambezi. With the present steam-boat it is quite impossible; the water is confined when low to a deep, narrow canal, as it passes the hills of Kauvabassa; it is then quite out of the question to attempt passing. Dr Livingstone and I have examined that region on foot, and found many rapids—one of great size, it seemed to be a fall of 30 feet at an angle of 30°. The only hope is at flood; then these rapids, being from 80 to 100 feet under the surface, become smooth; and what seems necessary is a boat with power sufficient to make headway in this deep part, for to pass among the shallows which then exist at the sides would be dangerous from cross currents and rocks. This part of the river is 30 miles in length. Dr L. has applied for another boat; if the Government grant it, we shall try what we can do during the next floods in December or March.*

This delay has been in so far fortunate, at least it has not been lost time. Some of the party are at Tette, working out the coal district. Dr. Livingstone and I have had a wandering time; we have been down to the sea; up Meramballa, a mountain near the Zambezi, in sight of Senna, of 4000 feet high, the summit and slopes a regular botanic garden, where during the ascent you pass, from the grassy river-banks in the first place, through forests with orchids, gingers, balsams, and ferns. As you ascend the vegetation changes; you meet with trees not so tall as those of the base, and Dr Livingstone observed that many were identical with those of the high lands of Louda in the west. We had here the Leucodendron and the common Pteris. On the top there is a great plateau, divided by ridges and peaks, with a varying elevation from 3000 to 4000 feet; it is well watered by springs, and the crops of maize are excellent. Lime trees grow wild in the woods. Here is a fine, cool, healthy climate, within sight of the town of Senna; yet it is doubtful whether the Portugese ever were there—it is a region quite unknown to them. The river Shire flows on the west side of this mountain; it is a fine river for navigation; we could get no information regarding it. The Manganja people, who dwell near the mouth, have been a complete barrier to the Portuguese, and likewise to those of the interior. The only means of transport by water is in canoes; and the Shire being deep and flowing quickly, with very few eddies of slack currents, it would be difficult to manage the unwieldy canoes of the country; and those in them, would be as completely in the power of the people as if they travelled overland. The case is otherwise with us; we pass with ease; no care is needed; there are few shoals, and the natives themselves soon see that their poisoned arrows would be nothing against guns when we are afloat out of range. We land daily to cut wood, and find that if one has no fear of them, there is no danger. They have never molested us, as we are the stronger; their poisoned arrows would be very little against rifles and revolvers. They have learned to distinguish between the Portuguese and English, and do not attempt the impositions they practise on the native hunters, such as taking a tooth and half the flesh of the elephants killed.

The Shire flows for 100 miles nearly north, in a plain of about 20 miles wide; there is a district near the middle which is marshy and cut up into islands, overrun by elephants; but the greater part is fine land for growth of cotton, sugar-cane, and rice. All these are now cultivated, and we can see at once the capabilities of the country. The cotton is of two sorts; one very fine in the good staple. The sugar-cane is chewed, but the people do not know the art of extracting the sugar.

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* We are happy to say that the Admiralty have granted a vessel.

Two crops of maize are obtained each year; and probably many other crops might be grown during the cold season, such as wheat. On either side are mountains; those of the north-east reaching 4000 feet. This would be a healthy position for Europeans. These high lands also yield crops of cotton, sugar-cane, and cereals, with various kinds of pulse, but more care is needed for their growth than in the valley. It seems a great thing to have this healthy region so near the coast, with a rich plain of enormous extent, and a navigable river leading, without obstruction, to the sea.

Dr Livingstone and I started again in April, to explore the region to the north, and following the Shire overland, for navigation was checked by a rapid, where it curves from among the mountains. We took with us a strong party of Makololos, so as to be independent of the natives, who will not dare anything unless against the weak. They are great cowards, unlike the Landeens and Kaffirs of the south. We had some idea of preparing the way for reaching the great lake, whence it seems probable the Shire takes its rise. For many days we wandered over a most rugged country. The people gave us no assistance, so that we often made a long road, which, had we known the general features of the country, would have been easy. However, it is only what all first explorers must expect. At length, however, we reached a plain which the river crossed to the east. For the 30 or 40 miles we had passed, the river flows between hills over a rocky bed, and is a series of cataracts, one after another. On reaching the plain, we struck across for a mountain opposite, called Dzomba, whence we hoped to have a view which might guide us in our future course. Here we met with native slave traders who, when they thought us Portuguese, looked on us with jealousy and without much fear; when they knew better, they seemed to expect that we should attack them and take off all the slaves. The English name is known far beyond where Europeans have ever penetrated. It took us long to cross this plain, although only 15 miles across. We were led astray by the people under the influence of these traders. We had in the end to take our own way, as we had done in the former part, and cross the hills opposite. Here we found a high plateau, with a totally new vegetation; a most interesting region, which I hope to explore more fully. The flora of Meramballa was but a slight indication of what we find here. To the east we had another plain, bounded on the other side by blue hills, and in it we could just distinguish a sheet of water. Our course now was for it. The information we received led us to believe that it was of great extent. On the 18th of April we got to the shore, and had before us one of the finest sights I ever beheld; an enormous expanse of water narrowing to the south, but reaching 30 miles in that direction; about 25 or 30 miles across to the north, we had a water horizon like the sea, and even from considerable heights nothing more was to be seen. There are in this lake many islands, with high mountains on them, and inhabited. The people tell us, that in a storm there are great waves, and we could see them breaking against one part of the shore. The water of this lake is bitter to drink; several rivers flow into it, but none out. The Shire never crosses the hills which we had passed, but keeps on the other plain, and is said to come from another lake, which they call the great lake or the Lake of the Stars, "Ninyessi."

This is called "Shirwa," and reaches to the north for at least 50 or 60 miles, being separated there from the "Ninyessi," by a piece of flat land, not many miles across. We waded in until the water reached our waists, in hopes of reaching a point whence we could take observations on the sea horizon, but the grass and reeds extended still farther, and we had to return covered with leeches. We had for guides then some of the slave party, and the people of the country said that they had led us off the proper path which goes to the bank in order to drown us. However, we got back all right; and that evening found it was in lat. 15° 23′ and long. 35° 35′. Having now seen this great lake, we thought we could not do much more at present. Here was a navigable inland sea, leading up to the great lake, of which rumours have for long reached us, and for which Captain Burton is now in search. We were rather anxious, too, for those in the vessel which we had left in the Shire, under the Quarter-master and Second Engineer, who were acting alone this trip.

On our return, we followed a different route, which took us over a high plateau, between 3000 and 4000 feet (the lake was 1800 feet above the Shire). This elevated region came down near to the Shire, and we found the path much easier than that by following up the river.

This seems to be a healthy part. We were out 23 days, and very seldom slept under cover; we were wet every morning with dew, and our clothes dried as we marched in the burning sun. Yet we were never delayed a day by the sickness of any one of the party, although often fatigued by evening with the heat and heavy road. The marches appear short when we came to correct them by observation; but they generally took us from sunrise to sunset, with only an hour to breakfast, and a rest of a few minutes about noon.

As to the geology of the country, it is all schist rocks, with a few spots of trap and porphyry. The strike is north and south. There is abundance of iron ore, which the natives reduce for knives, spears, and arrow heads. They also trade in hoes for cultivating the soil, with the neighbouring tribes.

The people are all "Manganja;" speak a modification of the language of Tette and Senna. The women are distinguished by the most repulsive of savage ornaments, a ring of ivory or bamboo, like a ring for a table napkin, in the upper lip; the lip being distended round the circumference, and projecting like a duck's bill.

Their religion is pure deism; they believe in a god and in medicine, or the ordeal which he directs as the means of discovering crime; if it cause vomiting, it shows innocence; if it acts by the bowels, crime, and they are put to death. But the doctors have a good knowledge of which to give, for there are different plants used. The only thing coming near to an idol which we heard of was the keeping the soul of their father in a basket, which they bring out when they get drunk with beer; but we could never get them to show it to us. When dead, they turn to lions and other beasts; only witches are made into crocodiles.

On our return, the Quartermaster was sick, but beginning to recover; he had been down with fever ever since we left. He is now better.

We are on our way to the mouth of the river to meet a man-of-war, with stores. We hear that the party at Tette have had a good deal of sickness; but the unhealthy season is quickly passing.

Daily exercise is absolutely necessary for health out here.

Dr. Livingstone and I have had fortunate health all along, although constantly in the most malarious districts, such as the Mangrove swamps of the Suabo, or the low lands of Senna and marshes of the Shire; out the whole day in the sun or rain. I believe the exercise more than counterbalances all these. One day when exposed to the sun, dissecting a young elephant, I found I could not stand it at all; had I been working I should not have felt it.

We are not without our own politics here, even in this outlandish place. The slave-trade goes on briskly from one of the mouths near Quillimane, to supply the demand at Bourbon. Those in power being of the French party, wink at it. The authorities are poorly paid, and have to make it up by other means. Trade is difficult; they lay themselves out for nothing but ivory and gold. They might have cotton and sugar, and that without the use of guano, as is required in Mauritius, and by the very hands they ship off to the French. The whole of Suabo, at the mouth of the river, is splendid cotton and cane land, and in the hands of the Portuguese. Those up the Shire are quite beyond their power; and even at Shupenya, near Senna, they have to pay tribute to the Landeens. They have last autumn finished the war with Mariano, who set himself up as independent; but there is still a great robber within a few miles of the town of Tette, which every canoe must pass on its way to Quillimane. We expect the Governor-General of the Province here immediately; he comes to establish his brother at Tette, as Governor of a new district which they call Zambezia, and which was formerly under that of Quillimane. We shall feel the want of Senhor Tito, the former commandant, who ought to have been made governor; he is the best man for it. We shall have to change our quarters, being at present established in the Residency at Tette.

2. *On the Morphological Import of Certain Vegetable Organs.*

By Christopher Dresser, Ph.D.

The author gave the results of his investigations into the morphological import of certain vegetable structures, especially those entering into the composition of the flower.

He commences his argument by contending that bud-scales, or *Perulæ*, are in many instances not metamorphosed leaves, but merely flattened petioles. He appeals to examples in *Acer* and *Æsculus*, where not unfrequently the bud-scales are furnished with small laminæ at their extremity, while they themselves remain unaltered. This proves, he considers, that the bud-scales are not metamorphosed or rudimentary entire leaves, but only represent petioles.

The tubercular papilla at the point of the normal bud-scale, and which is the first part that appears in its development, suffers what he terms a quasi-paralysis, or arrest of development; while, in the abnormal examples cited, this arrest does not take place, but the papilla proceeds to be developed into a lamina, as in the true leaf—the so-called transmutation in these instances resulting only from a more or less complete evolution of this papilla.

The author then endeavours to prove that the *Calyx*, in many instances, is a whorl of petioles—laminæ, in these cases, not entering into its composition. He refers, 1*st*, to the calyx of lavender, where one of the sepals develops a little lamina, which is more or less completely articulated to it; 2*d*, to the calyx of *Mussænda macrophylla*, where one sepal develops a lamina, while the other sepals, which are normal, are precisely parallel to the petiolar portion of the developed one; and lastly, to the monstrous calyx of a rose (which was exhibited), where from the sides and apex of the sepals, leaflets in various states of development were seen to spring, while the sepals themselves retained more or less completely their flat, phyllous, and conical normal form; in this instance the sepals are not *transmuted* into true leaves, but leaflets are developed upon their sides. This mode of reasoning is the same as that by which the *Phyllodium* of the acacia is universally regarded as a leaf-stalk, simply because a compound leaf is sometimes emitted from its apex.

Regarding *Petals*, the author adduces the following:—that petals continually become sepals in monstrous flowers, and this most commonly in flowers whose sepals have most manifestly a petiolar origin, as in roses; again, that in the *Caryophyllaceæ* flowers occur having petals with the most fully developed and clearly defined claws and limbs, while in those plants the leaves are so constantly sessile as to afford a characteristic of the race. From these circumstances he infers that in some cases, probably in roses, the petals result from petioles; whereas in other cases (as in the Caryophyllaceæ) they result from entire leaves. He does not, however, consider that, in these latter plants, the claws and limbs of the petals correspond to petioles and laminæ. "It seems contrary to reason to suppose that all the normal leaves of the plant should be sessile, as well as the leaves composing the outer floral envelope (the sepals); whereas the members of the inner floral envelope (the petals) should be raised upon long stalks."

Regarding the *Stamens*, the author urges arguments similar to those applied to the petals. The stamens may pass through the stages of petals and sepals, so "that whatever is the nature of the petal, such is the nature of the stamen also." Moreover, that in those plants with sessile leaves, the filaments and anther cannot correspond to petiole and lamina. He also refers to a monstrous stamen of *Tradescantia virginica*, where one-half of the stamen is converted into a petaloid member, which extends from the base of the filament to the summit of the anther, indicating that here the *whole stamen* corresponds to the sessile petal, and that there is thus no distinction, in this case, into petiole and lamina.

The author inclines to the belief that the carpel is in some cases equivalent to a petiole, from the fact that in certain cases monstrous carpels develop their ovules into rudimentary leaves. He does not, however, insist strongly upon this point, since he does not think it yet proved that ovules may not be true buds.

Sir Thomas Buchan Hepburn, in a letter to Dr Balfour, called attention to the mode in which *Taxodium sempervirens* sheds its leaves. The leaves themselves do not fall, but the small branchlets drop off, as if each branchlet was a pinnate leaf. Specimens were sent to illustrate this fact. The tree from which the specimens were taken was planted at Smeaton, a small seedling, in 1844, and is now about 28 or 29 feet high. Its mean annual growth for the last eight years, up to December 1858, has been about 2 feet 4 inches. It has not yet flowered.

SCIENTIFIC INTELLIGENCE.

BOTANY.

Distribution of Vegetable Species. By Professor Asa Gray.—A review of what has been published upon the subject of late years makes it clear that the doctrine of the local origin of vegetable species has been more and more accepted, although, during the same period, species have been shown to be much more widely dispersed than was formerly supposed. Facts of the latter kind, and the conclusions to which they point, have been most largely and cogently brought out by Dr Hooker, and are among the very important general results of his extensive investigations. And the best evidence of the preponderance of the theory of the local origin of species, notwithstanding the great increase of facts which at first would seem to tell the other way, is furnished by the works of the present De Candolle upon geographical botany. This careful and conscientious investigator formerly adopted and strenuously maintained Schouw's hypothesis of the double or multiple origin of species. But in his great work, the *Géographie Botanique Raisonnée,* published in the year 1855, he has in effect discarded it, and this not from any theoretical objections to that view, but because he found it no longer needed to account for the general facts of distribution. This appears from his qualified, though dubious, adherence to the hypothesis of a double origin, as a *dernier ressort,* in the few and extraordinary cases which he could hardly explain in any other way. His decisive instance, indeed, is the occurrence of the Eastern American *Phryma leptostachya* in the Himalaya Mountains. * * * *

I know not whether any botanist continues to maintain Schouw's hypothesis. But its elements have been developed into a different and more comprehensive doctrine, that of Agassiz, which should now be contemplated. It may be denominated the *autochthonal* hypothesis.

In place of the ordinary conception, that each species originated in a local area, whence it has been diffused, according to circumstances, over more or less broad tracts—in some cases becoming widely discontinuous in area through climatic or other physical changes operating during a long period of time—Professor Agassiz maintains, substantially, that each species originated where it now occurs, probably in as great a number of individuals occupying as large an area, and generally the same area, or the same discontinuous areas, as at the present time.

This hypothesis is more difficult to test, because more ideal than any other. It might suffice for the present purpose to remark, that, in referring the actual distribution, no less than the origin of existing species, to the Divine will, it would remove the whole question out of the field of inductive science. Regarded as a *philosophical* question, Maupertuis's well-known "principle of least action" might be legitimately urged against it, namely, "that it is inconsistent with our idea of Divine wisdom, that the Creator should use more power than was necessary to accomplish a given end." This philosophical principle holds so strictly true in all the mechanical adaptations of the universe, as Professor Pierce has shown, that we cannot think it inapplicable to the organic world also, and especially to the creation of beings endowed with such enormous multiplying power, and such means and facilities for dissemination, as most plants and animals. Why, then, should we suppose the Creator to do that supernaturally which would be naturally effected by the very instrumentalities which he has set in operation?

Viewed, however, simply in its *scientific* applications to the question under consideration (the distribution of plants in the temperate zone of the northern hemisphere), the autochthonal hypothesis might be tested by inquiring whether the primitive or earliest range of our species could possibly have remained unaffected by the serious and prolonged climatic vicissitudes to which they must needs have been subject; and whether these vicissitudes, and their natural consequences, may not suffice to explain the partial intermingling of the floras of North America and Northern Asia, upon the supposition of the local origin of each species. Let us bring to the inquiry the considerations which Mr Darwin first brought to bear upon such questions, and which have been systematically developed and applied by the late Edward Forbes, by Dr Hooker, and by Alphonse De Candolle.

No one now supposes that the existing species of plants are of recent creation, or that their present distribution is the result of a few thousand years. Various lines of evidence conspire to show that the time which has elapsed since the close of the tertiary period covers an immense number of years; and that our existing flora may in part date from the tertiary period itself. It is now generally admitted that above 20 per cent. of the mollusca of the middle tertiary (Miocene epoch), and 40 per cent. of the pliocene species on the Atlantic coast still exist; and it is altogether probable that as large a portion of the vegetation may be of equal antiquity. From the nature of the case, the direct evidence as respects the flora could not be expected to be equally abundant. Still, although the fossil plants of the tertiary and the post-tertiary of North America have only now begun to be studied, the needful evidence is not wanting.

On our north-western coast, in the miocene of Vancouver's Island, among a singular mixture of species referable to *Salix*, *Populus*, *Quercus*, *Planera*, *Diospyros*, *Salisburia*, *Ficus*, *Cinnamomum*, *Persoonia*, or other *Proteaceæ*, and a Palm (the latter genera decisively indicating a tropical or subtropical climate), Mr Lesquereux has identified one existing species, a true characteristic of the same region ten or fifteen degrees farther south, viz., the Redwood or *Sequoia sempervirens*. In beds at Somerville referred to the lower or middle pliocene by Mr Lesquereux, this botanist has recently identified the leaves of *Persea Carolinensis*, *Prunus Caroliniana*, and *Quercus myrtifolia*, now inhabiting the warm sea-coast and islands of the Southern States.* * * *

At length, as the post-tertiary opened, the glacier epoch came slowly on—an extraordinary refrigeration of the northern hemisphere, in the course of ages carrying glacial ice and arctic climate down nearly to the latitude of the Ohio. The change was evidently so gradual that it did not destroy the temperate flora, at least not those enumerated as ex-

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* These and other data, obligingly communicated by Mr Lesquereux, have been published in the May number of the American Journal of Science and Arts for 1859.

existing species. These, and their fellows, or such as survive, must have been pushed on to lower latitudes as the cold advanced, just as they now would be if the temperature were to be again lowered; and between them and the ice there was doubtless a band of subarctic and arctic vegetation—portions of which, retreating up the mountains as the climate ameliorated and the ice receded, still scantily survive upon our highest Alleghanies, and more abundantly upon the colder summits of the mountains of New York and New England—demonstrating the existence of the present arctic-alpine vegetation during the glacial era; and that the change of climate at its close was so gradual that it was not destructive to vegetable species.

As the temperature rose, and the ice gradually retreated, the surviving temperate flora must have returned northward *pari passu*, and—which is an important point—must have advanced much farther northward, and especially northwestward, than it now does; so far, indeed, that the temperate floras of North America and of Eastern Asia, after having been for long ages most widely separated, must have become a second time conterminous. Whatever doubts may be entertained respecting the existence of our present vegetation generally before the glacial era, its existence immediately after that period will hardly be questioned. Here, therefore, may be adduced the direct evidence recently brought to light by Mr Lesquereux, who has identified our live oak (*Quercus virens*), Pecan (*Carya olivæformis*), Chinquapin (*Castanæ pumila*), Planer-tree (*Planera Gmelina*), Honey-Locust (*Gleditschia triacanthos*), *Prinos coriaceus*, and *Acorus Calamus*,—besides an elm and a *Ceanothus* doubtfully referable to existing species—on the Mississippi, near Columbus, Kentucky, in beds which Mr Lesquereux regards as anterior to the drift. Professor D. D. Owen has indicated their position "as about 120 feet lower than the ferruginous sand in which the bones of the *Megalonyx Jeffersonii* were found." So that they belong to the period immediately succeeding the drift, if not to that immediately preceding it. All the vegetable remains of this deposit, which have been obtained in a determinable condition, have been referred, either positively or probably, to existing species of the United States flora, most of them now inhabiting the region a few degrees farther south.

If, then, our present temperate flora existed at the close of the glacial epoch, the evidence that it soon attained a high northern range is ready to our hand. For then followed the second epoch of the post-tertiary, called the *fluvial* by Dana, when the region of St Lawrence and Lake Champlain was submerged, and the sea there stood five hundred feet above its present level; when the higher temperate latitudes of North America, and probably the arctic generally, were less elevated than now, and the rivers vastly larger, as shown by the immense upper alluvial plains, from fifty to three hundred feet above their present beds; and when the diminished breadth and lessened height of northern land must have given a much milder climate than the present.—*Silliman's American Journal*, September 1859.

Remarks on the Botany of Japan, in its Relations to that of North America, and of other parts of the Northern Temperate Zone. By Professor Asa Gray.—It is interesting to notice that, notwithstanding the comparative proximity of Japan to Western North America, fewer of its species are represented there than in far distant Europe. Also—showing that this difference is not owing to the separation by an ocean—that far more Japanese plants are represented in Eastern North America than in either. It is, indeed, possible that my much better knowledge of American botany than of European may have somewhat exaggerated this result in favour of Atlantic North America as against Europe, but it could not as against Western North America.

If we regard the identical species only, in the several floras, the preponderance is equally against Western as compared with Eastern North America, but is more in favour of Europe. For the number of species in the Japanese column which likewise occur in Western North America is about 120; in Eastern North America, 134; in Europe, 157.

Of the 580 Japanese entries, there are which have corresponding

European representatives, a little above	8.48	per cent. of identical species,	0.27
Western N. American representatives, about	0.37	„ „ „	0.20
Eastern „ „ „	0.61	„ „ „	0.23

So geographical continuity favours the extension of identical species; but still Eastern North America has more in common with Japan than Western North America has.

The relations of this kind between the floras of Japan and of Europe are obvious enough; and the identical species are mostly such as extend continuously—as they readily may—throughout Russian Asia, some few only to the eastern confines of Europe, but most of them to its western borders. To exhibit more distinctly the features of identity between the floras of Japan and of North America, and also the manner in which these are distributed between the eastern and the western portions of our continent—after excluding those species which range around the world in the northern hemisphere, or the greater part of it, or (which is nearly the same thing in the present view) which are unknown in Europe—I will enumerate the remaining peculiar species which Japan possesses in common with America:—

In Japan.	*In W. N. America.*	*In E. N. America.*
Anemone Pennsylvanica		A. Pennsylvanica
(Coptis asplenifolia ?)	C. asplenifolia	
(Trautvetteria palmata)	T. palmata	T. palmata
Caulophyllum thalictroides		C. thalictroides
Diphylleia cymosa		D. cymosa
Brasenia peltata	[B. peltata]	B. peltata
Geranium erianthum	G. erianthum	
Rhns Toxicodendron	R. Toxicod., var.	R. Toxicodendron
Vitis Labrusca (Thunb)		V. Labrusca
Thermopsis fabacea	T. fabacea	
Prunus Virginiana ?		P. Virginiana
Spiræa betulæfolia	S. betulæfolia	S. betulæfolia
Photinia arbutifolia, in Bonin.	P. arbutifolia	
Pyrus rivularis ?	P. rivularis	
Ribes laxiflorum	R. laxiflorum	
(Penthorum sedoides, China)		P. sedoides
Cryptotænia Canadensis		C. Canadensis
Heracleum lanatum	H. lanatum	H. lanatum
(Archemora rigida ?)		A. rigida

In Japan.	*In W. N. America.*	*In E. N. America.*
(Archangelica Gmelini)	A. Gmelini	A. Gmelini
Cymopterus littoralis?	C. littoralis	
Osmorrhiza longistylis	O. longistylis	O. longistylis
Echinopanax horridus	E. horridus	
Aralia quinquefolia		A. quinquefolia
Cornus Canadensis	C. Canadensis	C. Canadensis
Viburnum plicatum		V. plicatum (lantanoides)
*Achillea Sibirica	*A. Sibirica	
*Artemisia borealis	*A. borealis	*A. borealis
Vaccinium macrocarpon	V. macrocarpon	V. macrocarpon
Menziesia ferruginea	M. ferruginea	M. ferruginea
(Boschniakia glabra?)	B. glabra	
*Pleurogyne rotata	*P. rotata	*P. rotata
(Asarum Canadense?)		A. Canadense
*Polygonum Bistorta	P. Bistorta	
Rumex persicarioides	R. persicarioides	R. persicarioides
Liparis liliifolia		L. liliifolia
Pogonia ophioglossoides		P. ophioglossoides
Iris setosa	I. setosa	
Trillium erectum, var.		T. erectum
(Smilacina trifolia)		S. trifolia
Polygonatum giganteum		P. giganteum
(Streptopus roseus)	S. roseus	S. roseus
Veratrum viride	V. viride	V. viride
Juncus xiphioides	J. xiphioides	
(Cyperus Iria)		C. Iria
Carex rostrata		C. rostrata
Carex stipata	C. stipata	C. stipata
Carex macrocephala	C. macrocephala	
Sporobolus elongatus	S. elongatus	S. elongatus
Agrostis scabra	A. scabra	A. scabra
Festuca pauciflora	F. pauciflora	
Adiantum pedatum	A. pedatum	A. pedatum
Onoclea sensibilis		O. sensibilis
Osmunda cinnamomea		O. cinnamomea
Lycopodium lucidulum		L. lucidulum
(Lycopodium dendroideum)	L. dendroideum	L. dendroideum

The names enclosed in parentheses are of species which I have not seen from Japan; some of them inhabit the adjacent mainland; some are imperfectly identified. Those marked * are high northern species in America.

Of those 56 extra-European species, 35 inhabit Western, and 41 Eastern North America. And 15 are Western, and not Eastern; 21 Eastern and not Western; and 20 common to both sides of the continent. Eight or ten of these 56 species extend eastward into the interior of Asia.

On the other hand, the only species which I can mention as truly indigenous both to Japan and to Europe, but not recorded as ranging through Asia, are

Euonymus latifolius, Fagus sylvatica, Blechnum Spicant,

Valeriana dioica, Streptopus amplexifolius, Athyrium fontanum,
Pyrola media.

Two of these species extend across the northern part of the American continent, and on to the Asiatic; another occurs on the north-west coast of America; and another, the *Fagus*, is represented in Eastern America by a too closely related species. It is noteworthy, that not one of these seven plants is of a peculiarly European genus, or even a Europæo-Siberian genus;—while of the fifty-six species of the Americo-Japanese region wanting in Europe, twenty are of extra-European genera; seventeen are of genera restricted to the North American, East Asian, and Himalayan regions (except that *Brasenia* has wandered to Australia); fourteen of the genera (most of them monotypic) are peculiar to America and Japan, or the districts immediately adjacent; one is peculiar to our north-west coast and Japan; and eight are monotypic genera wholly peculiar (*Brasenia* excepted) to the Atlantic United States and Japan. Add to these the similar cases of other American species (nearly all of them peculiarly Atlantic-American) which have been detected in the Himalayas or in Northern Asia,—such as *Menispermum Canadense* (*Dauricum*, DC.), *Amphicarpæa monoica ? Clitoria Mariana, Osmorrhiza brevistylis, Monotropa uniflora, Phryma leptostachya, Tipularia discolor ?* &c.—and it will be almost impossible to avoid the conclusion, that there has been a peculiar intermingling of the Eastern American and Eastern Asian floras, which demands explanation.

The case might be made yet stronger by reckoning some subgeneric types as equivalent to generic in the present view, and by distinguishing those species or genera which barely enter the eastern borders of Europe; e.g., *Cimicifuga fœtida, Mœhringia lateriflora, Geum strctum, Spiræia salicifolia,* &c.

It will be yet more strengthened, and the obvious conclusion will become irresistible, when we take the nearly allied, as well as the identical, species into account. And also when we consider that, after excluding the identical species, only 15 per cent. of the entries in the European column of the detailed tabular view are in italic type (*i.e.*, are *closely* representative of Japanese species); while there are 22 per cent. of this character in the American column.

For the latter, I need only advert to some instances of such close representation, as of

Trollius patulus	*by*	*T. Americanus,*
Aquilegia Burgeriana	"	*A. Canadensis,*
Rhus vernicifua	"	*R. venenata,*
Celastrus scandens	"	*C. articulatus,*
Negundo cissifolium	"	*N. aceroides,*
Sophora Japonica	"	*S. affinis,*
Sanguisorba tenuifolia	"	*S. Canadensis,*
Astilbe Thunbergii & Japonica	"	*A. decandra,*
Mitchella undulata	"	*M. repens,*
Hamamelis Japonica	"	*H. Virginica,*
Clethra barbinervis	"	*C. acuminata,*
Rhododendron brachycarpum	"	*R. Catawbiense,*
Amsonia elliptica	"	*Tabernæmontana*
Saururus Loureiri	"	*S. cernuus,*

and many others of the same sort,—several of which, when better known, may yet prove to be conspecific; while an equally large number could be indicated of species which, although more positively different, are yet no less striking counterparts.

To demonstrate the former proposition, I have only to contrast the extra-American genera common to Europe and Japan with the extra-European genera common to North America and Japan. The principal European genera of this category are—*Adonis, Epimedium, Chelidonium, Malachium, Lotus, Anthriscus, Hedera, Asperula, Rubia, Carpesium, Ligularia, Lapsana, Picris, Pæderota, Ajuga, Thymus, Nepeta, Lamium, Ligustrum, Kochia ? Daphne, Thesium, Buxus, Mercurialis, Cephalanthera, Paris, Asparagus*—to which may as well be added *Pæonia* and *Bupleurum*, the former having a representative on the mountains, and the latter in the arctic regions, of Western America, but both absent from the rest of our continent. Excepting *Pæderota* and *Buxus* (the latter a rather doubtful native of Eastern Asia), none of these genera are peculiar to Europe, but all extend throughout Asia and elsewhere over large parts of the world.—*American Journal*, September 1859.

Vegetable Hybrids.—In August 1858 M. Naudin fecundated the flowers of *Datura lævis* with the pollen of *Datura Stramonium.* He collected the seeds in October 1858, and sowed them in April 1859. The habit and general aspect of the plants which grew from these seeds resembled those of *D. Stramonium.* The flowers appeared later than those of the parents. The capsules for the most part were similar to those of *D. Stramonium.* In some, however, on the contrary, the characters of *D. lævis* were so decided that it was difficult to distinguish the capsules from those of that species. Again, in some of the fruits intermediate characters were seen,—the fourth, third, half, or three-quarters of the same fruit belonging exclusively to one or other of the parent species. Thus some of the capsules presented on one side a deep green surface, covered with sharp points, like those of *D. Stramonium*, whilst the other side exhibited the grayish tint and the smooth surface of the capsules of *D. lævis.* The influence, however, of *D. Stramonium* seemed to prevail most.—*L'Institut*, 9th Nov. 1859.

Vegetation of the Sea of Aral.—In 1857, the Imperial Academy of Sciences of St Petersburgh projected a zoological and botanical expedition to explore the steppes around the Sea of Aral, and placed it under the charge of two young naturalists, MM. Sévertzoff and Bortscheff. The first gentleman was killed in an encounter with the native tribes; but the expedition was continued by M. Bortscheff, and one of its more important results was the discovery, upon the north-east shore of the Sea of Aral, of a marine vegetation, exhibiting numerous species and entire families of plants (*algæ*), belonging exclusively to that which abounds in a deep sea, and which has never been met with in the salt or fresh water lakes situate in the interior. This discovery presents to botanists points of great geographical and historical interest, confirming the fact, in a manner not to be disputed, that the Sea of Aral is not a lake, but the bed of an ancient sea. It was previously known that its mollusca, if not identical, presented at least a great resemblance to those of the open seas; but botanists were until now ignorant of the existence of a marine flora upon its sea-bottom.—*Compte-rendu de la Soc. Imp. Geog. de Russe*, 1859, p. 53.

ZOOLOGY.

Egg of Dinornis.—An egg of the dinornis was dug up, together with a man's skull, at Kaikoras peninsula, New Zealand, about two years ago. It measured 10 inches in length by 7 in diameter, was of a dull white colour, old, brittle, and thin. In shape it was like a blunt hen's egg, and there was a hole, evidently purposely made to extract the contents, at one end, about half an inch in diameter. This egg, which I saw myself, belongs to Mr Fyfe of Kaikoras. Another was broken to pieces by the spade. The human skull was old in appearance, about one-sixteenth of an inch in thickness, the teeth still remaining perfect in it.—*Fred. A. Weld, Wellington, New Zealand.*

Polyps.—Van Beneden states:—1. The scyphistomes (*Hydra-tuba*) do not produce germs, but a part of their substance is transformed into medusæ. 2. The terminal segment, provided with the arm, is not detached on the form of scyphistomes (*Hydra-tuba*), endowed with the power of living elsewhere, but it becomes a medusa like the others, and the arms are absorbed on the spot in proportion as the medusa-form appears. 3. The pedicle of strobile shows a new crown of arms (*brachial corona*) before the first medusæ are detached. 4. The terminal medusa, bearing the arms which are absorbed, and preserving the mouth of the mother-scyphistome, does not present the same phenomena of evolution as the other medusæ, its sisters.—*L'Institut*, 9th Nov. 1859.

Remarks by Dr Daniel Wilson on a deformed fragmentary Skull, found in an ancient Quarry Cave at Jerusalem. Described by AITKEN MEIGS.—Dr Meigs, the able cataloguer of the Morton Collection of Crania, in the Cabinet of the Academy of Sciences of Philadelphia, embodies in an elaborate and careful thesis the results of an ingenious exhaustive process by which he has aimed at determining the race, by the form and characteristics, in a skull obtained under unusual circumstances. In 1857, Mr J. Judson Barclay presented to the Academy a human skull, in an imperfect condition, brought from a remarkable cave visited by him at Jerusalem, with the following results:—

Having received some information of the existence of a very extensive cave near the Damascus gate at Jerusalem (entirely unknown to Franks), Mr Barclay, in conjunction with his father and brother, resolved upon its exploration. Accordingly, having obtained permission to this effect, from the Nazir Effendi, they repaired to the cave, the mouth of which is situated directly below the city wall, and the houses on Bezetha. They found the wall at this spot about ten feet in thickness. Through a narrow, serpentine passage which traverses it, they gained an entrance into the cave. The length of the cavern they estimated at seven hundred and fifty feet, and the circumference upwards of three thousand feet. The roof is supported by numerous regular pillars hewn out of the solid limestone rock. The floor from the entrance to the termination forms an inclined plane, the descent of which is in some places very rapid. About a hundred feet from the entrance a very deep and precipitous pit was discovered, containing a human skeleton; supposed to be that of some unfortunate one who had fallen headlong down and broken his neck, or rather his skull, judging from the fracture which it exhibits. The bones, of almost giant proportions, gave evidence, from their decayed state, of having remained in that position for many years. The skull, unlike the rest of the skeleton, was in a remarkable state of preservation. Numerous crosses on the wall indicated that the devout pilgrim or crusader had been there; and a few Arabic and Hebrew inscriptions—too much effaced to be deciphered—proved that the place was not unknown to the Jew and the Arab. The explorers found many intricate, meandering passages, leading to immense halls as white as the driven snow, and supported by colossal pillars of irregular shape: some of them placed there by the hand of nature, others of them evidently by the stone-quarriers to prevent the tumbling in of the city. From their explorations the party concluded that this cavern and the Grotto of Jeremiah, two or three hundred yards distant, originally constituted one immense cave, which was formerly the great quarry of Jerusalem.

The cave appears, therefore, to be a very old one. An allusion to it under the name of the "Cotton Grotto" is made by Kadi Mejr-ed-din in an Arabic MS., entitled "The Sublime Companion to the History of Jerusalem and Hebron," and bearing date A. D. 1495. A gentleman who entered the cave subsequently to the visit of the Messrs Barclay, states, in the "Boston Traveller," that though its existence was long suspected, "nothing was positively known regarding it, as it has been kept carefully closed by the successive governors of Jerusalem. The mouth of the cavern was probably walled up as early as the times of the Crusades, to prevent its falling into the hands of a besieging army: earth was thrown up against this wall, so as effectually to conceal it from view, and it is only upon the closest scrutiny that the present entrance can be perceived."

The circumstances under which the skull was discovered afforded no clue to its ethnic classification; nor does its condition furnish any very decisive guide to the era to which it should be referred. It is confidently believed by those who have familiarised themselves with the minute characteristic details of comparative craniography, that by these alone ethnical types can be determined. A skull now in the collection of the Academy of Sciences at Philadelphia, and figured in Dr Meigs' Catalogue of Human Crania, No. 1352, as ancient Phœnician, was sent by M. Fresnel, the celebrated archæologist, to the late Dr Morton, without the slightest information as to where, or the circumstances under which, it was found. After careful study of its characteristics, Dr Morton pronounced it to be Phœnician. He afterwards learned from M. Fresnel that it was found in the sepulchral cave of Ben Djemma, in the Island of Malta, and probably belonged to an individual of that race, which, in the most remote times, had occupied the northern coast of Africa and the adjacent isles. It thus appears that Dr Morton, guided by osteologic characters alone, was enabled to announce the correct geographical locality of this skull, and perhaps also its true ethnic value; though of this latter point Dr Meigs expresses some doubts, arising from the remarkable resemblance which this skull bears to that of a wandering Chingà of Transylvania, depicted in Blumenbach's Decades (Tab. xi.) In like manner, some time before his death, Dr Prichard sent to Prof. Retzius two human crania, requesting an opinion as to the race to which they belonged. He pronounced one of them to be Roman, and the other Celtic, and was informed by Prichard that he was in all probability correct; for the two skulls had been dug up in an old battle-field at York, England, where the ancient British Celts had been vanquished by the Romans.

Encouraged by such examples of success, Dr Meigs proceeded to apply the tests which his experience in comparative craniology placed at his command. The skull, however, is peculiar, and, so far as his experience could guide him, unique. Among all the 1045 crania in the collection of the Academy, none presented a counterpart to it. Its most remarkable feature is that the occipital bone rises vertically from the posterior margin of the great foramen to meet the parietalia, which bend abruptly downward between their lateral protuberances. This striking peculiarity, therefore, gives to a skull brought from an ancient quarry-cave at Jerusalem some of the most typical characteristics of Peruvian crania. After minutely describing the appearance which the several bones present, Dr Meigs expresses his conviction that the head has been artificially deformed by pressure applied to the occipital region during youth; thus supplying an interesting illustration of the practice in the old world of the same custom of distorting the human head which was long regarded as peculiar to the American aborigines.

After marshalling all the probable ethnic claimants for this remarkable cranium, and assigning reasons for rejecting each, Dr Meigs shows that it unites some of the most characteristic elements of the Mongolian and Sclavonian head, while differing in some respects from both; and he finally concludes that it may be referred—not as a positive and indisputable conclusion, but as an approximation to the truth—to the people and the region about Lake Baikal. Through the Sclaves and Burats of that region, the short-headed races of Eastern Europe graduate apparently into the Kalmucks and Mongols proper of Asia; and here probably is a remarkable example of an artificially modified cranium of that transitional people of Lake Baikal.—*Canadian Journal*, Nov. 1859.

GEOLOGY.

Extract of a Letter from the late Mr James Motley, Engineer of the Julia Hermina Coal-mines of Borneo.

The letter from which the following is an extract was written shortly before the fearful massacre of Europeans at Kalangan, in Borneo, in which Mr Motley, his wife, and three children were murdered. Mr Motley superintended the working of the coal-fields in Labuan and Borneo, and was known to zoologists by the collections and notes which he forwarded to Mr Dillwyn of Swansea, and by the beautiful work which he published in conjunction with the latter gentleman—"Contributions to the Natural History of Labuan and the Adjacent Coasts of Borneo." One number only appeared in 1855, and unless Mr Dillwyn possesses materials for its continuation, we fear it will be the last. The paper alluded to he proposed to send to the care of Mr Thomson of Banchory, to be read at the Meeting of the British Association at Aberdeen; and the hints of a modern coal now forming in the China Sea and the Malay Waters, to the extent he mentions, may incite them to investigate this very interesting subject.

"I have already promised to send to the Natuur Kundige Vereinigung at Batavia a description of the Measures passed through in the pit, with a suite of specimens, which I have preserved, of every stratum passed through. But this will have an interest purely local, and, I think, ought rather to go to Batavia than anywhere else. On the fossils, considered alone, I am not able to write; because, though I can read plainly enough generally the date of which they tell, I cannot name them without books, which I do not possess, and have here no access to; and I have therefore proposed to send them to be named to Professor Bleekrode of Dordt. But I have been long preparing a paper upon another subject, of more general geological interest, and embodying some new facts, and, I think, new ideas, on the progress and growth of new coal-formations, now preparing for future ages. Such a growth is certainly going on here on a large scale, and I suspect that nobody has ever had so good an opportunity of observing it as myself. In the first place, I have been for some years engaged in working in a coal-formation—that of Borneo—which, though immeasurably older than that which I believe to be gradually filling up the China Sea, is yet as much younger than the coal-formations of Europe (of which I have also some experience), and therefore may be reasonably expected to yield points of analogy with present formations which might escape the keenest observer of secondary strata only; and secondly, I have had, more or less perfectly, the opportunity of observing the growth of the modern deposits at many points all round this great basin—namely, the west coast of Borneo, almost from north to south, several points on the east side of the Malay peninsula, and a great part of the east coast of Sumatra. I have here the opportunity—and it is a rare one—of seeing the cutting of our canal through some of these modern beds, never before disturbed by the hand of man; and I may add, that my knowledge of botany and natural history, though not very profound, is enough to give me the power of recording intelligibly inscriptions which the Great Author is now writing upon the new pages of the vast book of geology."

On the Genus Graptolithus. By James Hall.—The discovery of some remarkable forms of this genus during the progress of the Canada Geological Survey, has given an opportunity of extending our knowledge of these interesting fossil remains. Hitherto our observations on the Graptolites have been directed to simple linear stripes, or to ramose forms, which except in branching, or, rarely, in having foliate forms, differ little from the linear stripes. In a few species, as *G. tenuis* (Hall), and one or two other American species, there is an indication of more complicated structure; but up to the present time this has remained of doubtful significance. The question whether these animals in their living state were free or attached, is one which has been discussed without result; and it would seem to be only in very recent times that naturalists have abandoned altogether the opinion that these bodies belonged to the *Cephalopoda.*

In the year 1847 I published a small paper on the Graptolites from the rocks of the Hudson River group in New York. To the number there given, two species have since been added from the shales of the Clinton group. Other species, yet unpublished, have been obtained from the Hudson River group; and since the period of my publication in 1847, large accessions have been made to our knowledge of this family of fossils, and to the number of species then known. The most important publications upon this subject are, "Les Graptolites de Bohême," par J. Barrande, 1850; "Synopsis of the Classification of British Rocks, and Descriptions of British Palæozoic Fossils," by Rev. A. Sedgwick and Frederick M'Coy, 1851; "Grauwacken Formation in Sachsen, &c.," by H. B. Geinitz, 1852.

The radix-like appendages, known in some of our American as well as in some European species, have been regarded as evidence that the animal in its living state was fixed; while Mr J. Barrande, admitting the force of these facts, asserts his belief that other species were free. It does not however appear probable that in a family of fossils so closely allied as are all the proper *Graptolitidæ*, any such great diversity in mode of growth would exist.

Heretofore we have been compelled to content ourselves, for the most part, with describing fragments of a fossil body, without knowing the original form or condition of the animal when living. Under such circumstances, it is not surprising that various opinions have been entertained, depending in a great measure upon the state of preservation of the fossils examined. The diminution in the dimensions, or perhaps we should rather say in the development, of the cellules or serrations of the axis towards the base, has given rise to the opinion advanced by Barrande, that the extension of the axis by growth was in that direction, and that these smaller cells were really in a state of increase and development. In opposition to this argument, we could before have advanced the evidence furnished by *G. bicornis*, *G. ramosus*, *G. sextans*, *G. furcatus*, *G. tenuis*, and others, which show that the stripes could not have increased in that direction. It is true that none of the species figured by Barrande indicate insuperable objections to this view; though in the figures of *G. serra* (Brong.), as given by Geinitz, the improbability of such a mode of growth is clearly shown.

It is not a little remarkable that, with such additions to the number of species as have been made by Barrande, M'Coy, and Geinitz, so few ramose forms have been discovered; and none, so far as the writer is aware, approaching in the perfection of this character to the American species.

Maintaining as we do the above view of the subject, which is borne out by well-preserved specimens of several species, we cannot admit the proposed separation of the Graptolites into the genera *Monograpsus*, *Diplograpsus*, and *Cladograpsus*, for the reason that one and the same species, as shown in single individuals, may be *monoprionidean* or *diprionidean*, or both; and we shall see still farther objections to this division, as we progress, in the utter impossibility of distinguishing these characteristics under certain circumstances. We do not yet perceive sufficient reason to separate the branching forms from those supposed to be not branched, for it is not always possible to decide which have or have not been ramose among the fragments found. Moreover, there are such various modes of branching, that such forms as *G. ramosus* present but little analogy with such as *G. gracilis*.

Mr Geinitz introduces among the *Graptolitideæ* the genus *Nereograpsus*, to include *Nereites*, *Myrianites*, *Nemertites*, and *Nemapodia*. Admitting the first three of these to be organic remains, which the writer has elsewhere expressed his reasons for doubting, they are not related in structure, substance, or mode of occurrence, to the Graptolites, at least so far as regards American species; and the *Nemapodia* is not a fossil body, nor the imprint of one, but simply the *recent traces of a slug* over the surface of the slates. The genus *Rastrites* of Barrande has not yet been recognised among American *Graptolitideæ.* These forms are by Geinitz united to his genus *Cladograpsus,* the propriety of which we are unable to decide.

The genus *Gladiolites* (*Retiolites* of Barrande, 1850; *Graptophyllia* of Hall, 1849) occurs among American forms of the *Graptolitideæ* in a single species in the Clinton group of New York. A form analogous with the reticulated margins and straight midrib has been obtained from the shales of the Hudson River group in Canada, suggesting an inquiry as to whether the separation of this genus, on account of the reticulated structure alone, can be sustained. In the meantime we may add that the Canada collection sustains the opinion already expressed, that the *Dictyonema* will form a genus of the family *Graptolitideæ.* The same collection has brought to light other specimens of a character so unlike anything heretofore described, that another very distinct genus will thereby be added to this family. The Canadian specimens show that the Graptolites are far from always being simple or merely branching flattened stems.

The following diagnosis will express more accurately the character of the genus *Graptolithus:*—

Genus GRAPTOLITHUS (Linn.)—*Description.*—Corallum or bryozoum fixed (free?), compound or simple, the parts bilaterally arranged, consisting of simple stripes or of few or many simple or variously bifurcating branches, radiating more or less regularly from a centre, and in the compound forms united towards their base in a continuous thin corneous membrane or disk formed by an expansion of the substance of the branches, and which in the living state may have been in some degree gelatinous. Branches with a single or double series of cellules or serratures, communicating with a common longitudinal canal, affixed by a slender radix or pedicle from the centre of the exterior side.

The fragments, either simple or variously branched, hitherto described as species of *Graptolithus,* are for the most part to be regarded as detached portions from the entire frond.

In the living state we may suppose those with the corneous disks and numerously branched fronds to have been concavo-convex (the upper being the concave side), or to have had the power to assume this form at will. In many specimens there is no evidence of a radix or point of attachment, and they have very much the appearance of bodies which may have floated free in the ocean.

Exploration of British North America. Report on Geology by Dr HECTOR.—Fort Edmonton, Saskatchewan, January 10, 1859.—I have the honour to make the following report of my geological observations during the past season, in which is embodied only the principal results and general features of the country examined, the details being reserved for a more elaborate study and comparison than can be executed here. On starting from Fort Carlton on 14th of June 1858, we crossed the low track of prairie land which is bounded to the west by that line of high ground which has been traced from longitude 103° W. sweeping to the

N.W. to meet the south branch of the Saskatchewan at the elbow, known as the "Coteau des Prairies," and from that point being continued to the north branch as the Bad Hills and Eagle Hills, while across that river it reappears as the Thickwood and White Lake Hills. The average elevation of these plains above Carlton (which is built upon the first river level, thirty-five feet above the water) is 250 feet, or 2125 feet above the level of the sea, and on it rests isolated portions of the higher level which have survived the general denudation, rising as rounded hills from 300 to 400 feet in height, such as Moose Hill on the south branch, and the two Minetonass Hills (Creefor Hill by itself), one of which is opposite to Carlton and the other to Forte à la Corne. These plains are plentifully strewn with erratic blocks of all sizes, being fragments of the rocks of the Granitic belt, which runs to N.W. from Lake Superior to the Arctic Sea, with others of Magnesian limestone and buff-coloured quartzose rock of Silurian age, which crops out all along the western flank of that range. A very remarkable line of the Magnesian limestone boulders occurs at the distance of twenty miles above Carlton, crossing the country from the Thickwood Hills in a southerly direction, towards the Moose Hills on the south branch. This limestone contains the same indistinct fossiliferous markings as that at the Stoney Hill behind Fort Garry. Some of these masses are of immense size, being made up of portions of several beds which only loosely cohere to form the block. They are all sub-angular, without any glacial markings, although some have their sides highly polished and smoothed from the buffalo rubbing against them. One of these blocks was measured, and computed to be 140 tons. The nearest known point where this limestone occurs *in situ*, from whence these blocks may have been derived, is 170 miles distant to N.E. Disregarding, for the sake of clearness, the order in which the country was examined, I now give at once an account of the whole "drift" phenomena observed. As we travelled to the west, the drift was found to preserve the same mineral character of variable proportions of sand and clay, having boulders interspersed, but chiefly with the clay predominating. The boulders, however, decrease in size, and those of limestone become very rare as the higher plains are gained. At Fort Edmonton, for instance, I found it difficult last winter to procure fragments with which to make lime for medicinal purposes, although the river bed is strewn with those of other rocks. Its depth also becomes much less, forming only a superficial covering to older strata, when observed in the river sections to the west of the Eagle Hills. As we approached the Rocky Mountains it quite disappears from the table-lands, and is only to be found in depressions of the plain through which streams run; and even the existence of true drift in these places is rendered doubtful, owing to the prevalence of more recent deposits, which have been formed of its rearranged materials. At the altitude of 4000 feet above the sea, and at the distance of fifty miles from the mountains, there however occurs a very extraordinary group of blocks of granite, resting upon a high plateau, formed of sandstone strata, to be afterwards mentioned. These blocks are of great size, one having been estimated to weigh 250 tons. Although lying miles apart, they seem to consist of the same rock—viz., a mixture of quartz with red felspar, the latter predominating, with only faint traces of mica disseminated in very minute flakes.

No granitic rocks have been met with on this side of the watershed of the mountains, and it is not probable that any such exist, at least between the two branches of the Saskatchewan. These blocks present smooth surfaces, although in general they are rhomboidal in form. Some are cracked into several pieces, which are quite detached, but have evidently at one time formed part of a whole. If these blocks were derived from the granitic belt to the east, as I believe all the other boulders on the plains to have been, then they must have travelled at least from 400 to 450 miles. From the fact, however, that they are almost on the western verge of the drift deposit, and that the boulders imbedded were found as a rule to diminish in size in that direction, it may be that the presence of these large blocks is due to very different agencies, different at least in the time of their occurrence. Close in, along the base of the mountains, neither on the high plateaus nor in the profound valleys by which these are traversed was there observed any traces of the drift, or its dispersed erratics. Within the outer range of the mountains, which are comparatively low, and wooded to their summit, the valleys are occupied by immense deposits of rounded shingle, composed of fragments of the various rocks which have been found to compose the mountains. This shingle, which in some places is loose, and mixed with a large proportion of sand and gravel, in others is cemented by calcareous matter into a solid conglomerate. It fills up the valleys not only along the edge of the mountains, but also right into their interior, forming beautifully marked terrace levels along the streams. This is well exhibited on the north branch of the Saskatchewan, where these deposits skirt its wide valleys for nearly seventy miles of its course through the mountains, expanding where it widens so as to form extensive plains, as at the Kootanie plain, and always affording a margin of level ground along the river, rendering the road very practicable. Towards the upper ends of the valleys the calcareous matter of these deposits so increases as to replace altogether the shingle, when it becomes a fine gritty calcareous mud, of glistening whiteness. This same deposit has a much larger development in the valleys on the west side of the watershed, forming terrace levels in exactly the same manner. I observed no shingle beds with it there, however,—these apparently being replaced by fine sand and gravel. In the valley of Bow River, there is much less of this calcareous matter in the deposit, it having more of a loose sandy nature, and except at the entrance to the valley in the neighbourhood of the Bow Fort, rarely exhibiting the terrace levels. In the smaller gorges, where streams come down from the mountains, it is replaced by an angular "breccia," of which patches cling in the most singular positions. This latter deposit is most likely of the nature of glacier moraines, although it is found where no glacier occurs anywhere in the neighbourhood. I found, however, that the glaciers in the chain had at one time extended a considerable degree beyond their present limits, and therefore, at that time they possibly may have existed in portions of the mountains where now there are none. The terrace deposits seem to reach pretty nearly the same altitude in different parts of the mountains—viz., about the height of 1000 feet above the level of the plains at their eastern base. I found that, in crossing the different heights of land, the easiness of the pass corresponded with the degree to which these deposits had remained untouched, owing to peculiarities in the form of the valleys. In the case of every height of land, whether of those examined by Captain Palliser or by myself, with the single exception of the Vermilion Pass, the slope is gradual to the east, but to the west the descent is with extreme rapidity. This arises from these deposits having being scooped out close up to the rocky nucleus of the height of land, by currents acting from the western side of the chain, while on the east the erosion has been much more feeble. How much this may depend on the difference between the width of the valleys which pass through the flanking chains on the east side of the height of land from those on the west I am not prepared to say, until the nature of the country to the west has been ascertained. Currents acting on the chain while submerged, would of course be greatly modified in their action by any such differences. Respecting the age of these deposits I am in doubt. They extend towards the east along the river valleys—at least shingle deposits of the same nature are found at a considerable distance from the mountains, in the valleys of the north and south branches, and of the Red Deer River. Its relations to the drift have not been distinctly ascertained, as the boulders which mark its presence are only in that district of country found on rounded knolls away from the rivers.—*Papers relative to the American Exploration*, 1859.

PUBLICATIONS RECEIVED.

L'Institut for August, September, October, and November, 1859.—*From the Editors.*

Natural History Review for October 1859.—*From the Editors.*

Proceedings of the Manchester Philosophical Institution—(continued).—*From the Society.*

Natural History of the European Seas, by Prof. E. Forbes and R. G. Austen.—*From the Publishers.*

Journal of the Asiatic Society of Bengal, No. 2, for 1859.—*From the Editor.*

Canadian Journal of Industry, Science, and Art, for November 1859.—*From the Editors.*

Reply to Sir David Brewster's Memorial to the Lords Commissioners of Her Majesty's Treasury on the New Series of Dioptric Lights, by D. and T. Stevenson, Civil Engineers.—*From the Publishers.*

Martins, Charles, Du Froid Thermométrique et de ses Relations avec le Froid Physiologique dans les Plaines et sur les Montagnes.—*From the Author.*

THE

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*On Gebel Haurân, its adjacent Districts, and the Eastern Desert of Syria, with Remarks on their Geography and Geology.** By JOHN HOGG, M.A., F.R.S., F.L.S., &c.

The Rev. J. L. Porter, M.A., F.R.G.S., being for several years a resident missionary in Damascus, took the opportunity in February 1853 of visiting the Lejah and the Haurân ; and he gave to the world a very interesting account of the same in his amusing work, entitled, "Five Years in Damascus ;" which was published two years afterwards. In the spring of 1857, my friend, Mr Cyril Graham of Trinity College, Cambridge, became acquainted with Mr Porter in Syria, and having heard much from him respecting the Haurân, he resolved to go and explore it. Accordingly, in the summer of that same year, Mr Graham travelled through it. But, since that remarkable region is already known by the able descriptions of Porter, Buckingham, Burckhardt, and Seetzen, it will be unnecessary for me to add any particular account of it, except a short notice of its geology. That portion itself chiefly comprised, under the Arabic name of the Haurân, the Auranitis of the Greeks, will, in Mr Graham's words,—" Ever claim the most solemn interest, being the old Land of Bashan (of which Batanæa of the Romans was a part), the country of that most remarkable people, the 'Rephaim,' who occupied this land long before Abraham crossed the Desert, and among whom, in later times, Og, the king of Bashan, was one of the greatest chiefs."* Mr Graham states, at the present day, the Druzes, who are of the same stock as those in the Lebanon, "are the settled inhabitants of Bashan. They live in the very cities out of which, more than 3000 years ago, the Rephaim were expelled, through the help of God, by the victorious Israelites." He much esteems them for their great kindness and hospitality; and he reckons the entire number of the *men only* who dwell in the Haurân at 7000.

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* This paper was in part read to the Geographical Section of the British Association, at the meeting held in Aberdeen, on September 15, 1859.

But other portions, lying mostly to the east and north of Gebel Haurân, as they were unvisited before the year 1857 by modern European travellers, and were only imperfectly known by the rough descriptions afforded by Arab guides, shall now be briefly noticed. And I may mention that, as Mr Porter had in the year 1854 kindly favoured me with the unpublished Greek inscriptions, which he had copied in his tour through the Haurân, and which I edited in the Transactions of the Royal Society of Literature in the following year; so Mr Graham very handsomely intrusted me last year with the publication of about forty more Greek inscriptions, which he met with in that country, and which have appeared only a few weeks ago in this year's Part of the Transactions of the same Society. They are also accompanied with some notes, which I have incorporated in the present communication.

Mr Graham passed from Shuhba, at the south-eastern border of the Lejah (Trachonitis), through Wadi Nimreh (*i.e.* 'Valley of the Panthress') and several ancient towns, the most considerable of which "was Malkyeh," near the edge of the Desert. "On the wall," he says, "of a public building there, I found a Greek inscription, from which it appears the Greek name was likewise 'Malkaia,'" or probably Malcova. The word on the inscription itself, according to Mr Graham's transcript, looks more like ΜΑΛΚΟΥΗΝΟΥ, "of an inhabitant of Malcova." As yet, however, I have not been able to find such a name in any work on ancient geography.

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* See Mr Graham's "Explorations in the Desert of the Haurân, and in the Ancient Land of Bashan," p. 228, in the Journal of the Royal Geographical Society, vol. xxviii. Lond. 1859.

Proceeding to the east, Mr Graham came to the southern extremity of the Safah, from which the chain named Tell-e' Safah* rises. It is an insulated volcanic district, which is elevated above the Desert. Here were found inscriptions in an unknown Shemitic character, accompanied with representations of palm-trees and other figures roughly cut in large blocks of basalt. On the east side of the Safah were some ruins of towns, similar to the ancient cities of Bashan, though remaining in a less perfect condition. The hills rising from the Safah are described as at distances which vary from four to ten miles from the border.

Continuing in a south-easterly course, the traveller crossed a tract of country called El Harrah, which Mr Graham says is probably derived from Harr, "heat." Now, the word Harr, I find, is still more indicative of the *actual nature* of that district, inasmuch as it strictly means, "*heat reflected* from the ground." This entire tract is thought to extend five days' journey in length, and perhaps a day and a quarter's journey in width. At length he came to a wadi, known as El Warran (The Lizard), where were more ruins and Shemitic inscriptions. Turning westward, he passed close to Tell Ozda, a solitary hill on the east of Tell-e' Nemareh (The Hill of the Panthers), and then entered a broad wadi. Indeed, at this latter spot, Mr Graham "found the greatest number of inscriptions on the basalt stones that he had met with anywhere." The characters of these Shemitic inscriptions, he thinks "most nearly approach to those of the Himyaritic;" and he states that they are to appear in the Transactions of the Royal Asiatic Society. It was at first supposed, and in fact Mr Porter in his letter to me from Damascus, in January 1858, announcing the discovery of these remarkable inscriptions, mentioned expressly that they resembled the Sinaic inscriptions, but I think there is nothing to warrant such conclusions.

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* See an Explanation for thus writing E' Safah, in note. p. 194, vol. xlviii. Edinburgh New Philosophical Journal, to my former Memoir on the Geography and Geology of the Sinaic districts.

Mr Graham observes, " The absence of all Greek inscriptions," *except* four found at Nemâreh, " seems to argue that *this* country (*i.e.*, I suppose the whole of it) never came under the dominion of the Greeks," or of the Romans; and he adds, " whether this country once was tributary to Phœnicia, or whether we have on these stones inscriptions of a far earlier period—traces perhaps left by the old Rephaim themselves, who first occupied this land,—is at present mere matter for speculation."*

In the centre of this broad wadi, or river-bed, is E'Nemâreh, where, on a high mound, are situated some ancient buildings. " These, again, closely resembled those old houses of Bashan, with the beams of stone, and doors still perfect."

E'Nemâreh appears to me to derive its name from a female saint, whose shrine is there, and who is called by the Arabs " Nimreh bint e' Nimur," which means, " panthress, daughter of panthers." It would be extremely interesting if we could trace this saint, or goddess, who has been for long venerated by the Arabs, to the worship of the ancient Phœnicians; for it seems not improbable that this "Panthress" might bear an allusion to the great Phœnician deity, Ashtoreth, who was identical with Astarte, whom the best Grecian authorities accounted no other than Diana, the goddess of hunting; and indeed, at an early period, most of the Syrian races, as well as the Arabians, previous to the age of Mahomet, worshipped the famous goddess Ashtoreth. This suggestion may hereafter be more fully investigated, when the unknown oriental inscriptions which are so numerous near Nemâreh shall have been interpreted.

From the ruins of E' Nemâreh—Mr Graham being unable to explore the desert any further to the east, which he describes as "a vast plain bounded only by the horizon, and which reaches, it seems, without a single break to the Euphrates"—returned to Shuhba, near the northern base of Gebel Haurân.

He then visited the ruins of several towns situated on the east and south of the Gebel Haurân range, being much of it new country, namely Nimreh, an old town built on a hill just above the Wadi Nimreh; there he noticed a Roman temple, but not any inscription; also Bshennef, Busân, Sali, and Sehwet El Khudr. The first, or Bshennef, "is beautifully situated on the borders of a wild glen which leads into the great plain below." He thinks that Bshennef must have been a place of much importance, not only judging from the house-doors, which were more than usually ornamented, but also from a beautiful temple which he saw there.

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* Journal of the Royal Geographical Society, vol. xxviii. p. 241.

The second—whose name Busân has no connection, as one might naturally suppose, with that of Basan or Bashan, but it is thought to be abbreviated from Abu Sân, or "Father Sân"—is a large town, "its streets are very regularly built, its stone houses perfect, and it commands an extensive view of the desert."

The third, Saleh, or Sali, is "another very large town on the mountain," and possesses many fine springs, to which the Arabs bring their flocks and camels to drink. Sehwet El Khoudr (or St George) was, I believe, first visited by Burckhardt. This portion of Bashan Mr Graham describes as "very beautiful," and covered with forests of oak. Below the picturesque and conical peak El Kuleib ("the little heart") lies on the south the ancient city named Kufr—"the town gates of which, composed of two large slabs of stone nearly 9 feet high and a foot or more in thickness, are still standing uninjured." Afineh, Hejmar, and Ari, possessed nothing remarkable.

From Bozrah, now called Busrah, Mr Graham went south, and passed through several remains of ancient cities, and was fortunate in reaching the extensive ruins of Um El Jemâl, considered most probably the Beth Gamul of Scripture. I will not here describe these places, as they are beyond the proposed bounds of the present communication. Afterwards he came to Orman, which had been identified by Burckhardt, from some Greek inscriptions which he had found there, as "Philippopolis," a city founded by the Roman Emperor Philip, who was a native of Busrah in this portion of Syria, which is often styled in ancient authors Arabia. Proceeding thence eastward, he arrived at a still more important place, named

Malah, the former name of which he could not determine, and following an ancient road, he made *Deir E' Nasrani*, signifying "Convent of the Christians," the limit of his excursion to the eastern desert.

This enterprising traveller "has no doubt that the towns in this country, like those of Bashan, are of the highest antiquity," and that these were "the cities of the giant *Rephaim.*" This people being naturally of a *gigantic* size; the word "Rephaim," which strictly meant the *Nation*, was used in time to signify any *giants;* and it has sometimes created, as he observes, a "confusion in our translation of the Old Testament." In the third chapter of Deuteronomy, v. 11, our version has translated that Og was "of the remnant of *giants*," and not, as is clearly more correct to read, "of the remnant of *the Rephaim*," "ha Rephaim" being the original in the Hebrew text. So the Septuagint translation has in the same verse, τῶν Ῥαφαΐν, and the Alexandrine, τῶν Ῥαφαέιν; but the Latin Vulgate makes the like mistake as the English Bible, in rendering it "gigantum." At a later period the Romans colonized, enlarged, and beautified most of those strong and "walled cities" which are still remaining in good preservation, although long ago abandoned and uninhabited; they thus prove, in a remarkable manner, the fulfilment of Jeremiah's prophecy, "The spoiler shall come upon every city, and no city shall escape;" and "the cities thereof shall be desolate *without any* to dwell therein."

Next, I will more particularly describe the region to which I am referring, and then add a few notes on the chief *geographical* and *geological* features of the portion of Syria or Northern Palestine included in it. The region comprises a district from Busrah about 36° 26′ 45″ to 37° 45′ nearly longitude east from Greenwich, and from Salkhad and E' Deir south of Busrah, about 32° 30′, to nearly the supposed centre of Bahret Hijâneh, the southernmost of the three lakes in the territory of Damascus, in 33° 20′ north latitude. And I may here remark, that the most recent maps of Syria do not agree as to the exact positions of Damascus and Busrah; for in Mr Porter's *first* map, which I had the pleasure of communicating to the Royal Geographical Society in November 1855, which is most neatly engraved by Mr Arrowsmith, and published in the 26th volume of their "Journal," Damascus is placed in just about 36° 17′ 15″ east longitude, and in 33° 31′ 15″ nearly north latitude, whilst Busrah is laid down in about 36° 26′ 35″ east longitude, and in 32° 32′ 20″ north latitude; and the same positions appear to be given in the small map which accompanies Mr C. Graham's paper in volume xxix. of the Journal of the Geographical Society, which is only just published. But in the map by Henry Kiepert, engraved in Mr Porter's "Handbook of Syria," published in 1858, the city of Damascus is laid down in 36° 16′ 40″ east longitude, and in 33° 31′ 40″ north latitude; but Busrah is placed in 36° 22′ 30″ east longitude, and in north latitude 32° 31′ 40″; thus giving a difference nearly as to Damascus of 35″ of longitude and 25″ of latitude; and about a difference as to Busrah of 4′ 5″ of longitude, and of 40″ of latitude.* My map I have drawn on a scale *eight times larger* than that of Mr C. Graham's map so neatly executed by Mr Arrowsmith.

In the left hand, or north-west corner, appears a small part of the territory of Damascus called Wadi El Ajam, or the valley of the Awaj—the ancient river Pharpar which runs into the lake Hijâneh at its north point, and whose waters are there mixed with those of the Liwa in the rainy season, which enters the lake at its southern extremity, descending from its sources near Nimreh at the northern roots of Gebel Haurân. The exact size and borders of this lake are as yet undetermined, but it is thought to be between five and six miles in length, and four or five in breadth.

Then succeeds the Lejah, the former Trachonitis of the Romans and the Argob of the Bible; this territory skirts the western side of Gebel Haurân, west of Suweideh to the Wadi Zêdy, or a little further south, and at a short distance from

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* From Van de Velde's "Map of the Holy Land," published at Gotha in 1858, I make Damascus to be placed in just about 36° 15′ 40″ east longitude, and 33° 31′ 30″ north latitude; and Busrah, 36° 26′ 35″ longitude, and 32° 29′ 0″ latitude;—but in his explanatory book, he writes the positions respectively as—Damascus (Castle) 36° 15′ 30″ longitude, and 33° 31′ 20″ latitude; and Busrah, according to Porter, 36° 26′ 30″ longitude, and 32° 29′ 40″ latitude; and according to Arrowsmith, 36° 27′ 15″ longitude, and 32° 35′ 30″ latitude.

Busrah the southern corner of the Haurân, the former Auranitis, and a province of the Scriptural Bashan, intervenes. The extensive and chiefly mountainous district from about Salkhad on the south to Tell El Asfar on the north, is the present Bathanyeh, and the Batanæa of the Romans, which formed another province of the ancient kingdom of Bashan. Of this, from about Ayûn to near Shuka extends the fine range of lofty hills called Gebel Haurân, and Gebel E' Druz—the mountain Alsadamus of Ptolemy. On the east and north of this a vast plain stretches out; that portion of it on the east being of vast extent, is called El Harrah, belonging in former times to what was generally named the Great or Arabian Desert, whilst the other portion on the north is broken by the hills E' Tellul, and the still higher ridges of E' Safah. The north and far east of the latter are desert regions at present unknown and unvisited by any modern European traveller.

The *geology* of these districts is but little made out; and the remarks which I now briefly present are mostly taken from the accurate accounts of Messrs Burckhardt, Porter, and Graham.

The south-eastern extremity of Wadi El Ajam, a part of the territory of Damascus, with the Lake or Bahret Hijâneh on the east, is an extensive level, and the river Awaj or Pharpar flows through alluvial and diluvial beds, which intervene between the parallel lines of dark hills, Gebel El Aswad and Gebel Mania. The white chalk, or marl, or limestone formation filled with fossils, terminates a little north of them; for Gebel El Aswad, or the "Black Mount," affords the quarries from which are taken the *black* paving stones used in Damascus. Also, Gebel Mania is characterised as "*dark* and bare" in aspect, its most lofty point being a truncated, cone-shaped peak. From that spot the volcanic rock and basaltic boulders succeed to the east and south, and prevail over a vastly extended district. In fact, from thence to the northern slopes of the Haurân range, the country presents a fine but somewhat undulating plain, about twenty-five miles long. At the north of the Wadi Liwa commences that remarkable region now known as the Lejâh, answering to the Argob of the Bible, and the Trachonitis of the Romans, both of which names denote its physical character, in meaning the *rough*, *stony*, or a *mass of stones*. The southern portion of it runs down in a narrow strip along the entire western base of Gebel Haurân to about Busrah, the former *Bostra*, to which city, according to Eusebius, it extended. For nearly half this length from the north, the Wadi Liwa runs towards its eastern boundary. Through this valley, which is about forty-five feet in depth, having its banks broken and rocky, a considerable stream flows in the rainy season, and then makes the Bahret Hijâneh a large lake, which in summer is only a morass covered with reeds. South of the lake a broad plain stretches out to the lofty Tell Khaledyeh, on the summit of which are some ruins. Parts of the plain appear to have been divided into fields by stone fences, the stones having been gathered together; and more towards the Liwa are many towns, half ruinous and quite deserted; the walls of the houses having been built of black basalt of vast thickness. The ground thence forms a gradual slope to the heights at Shuka, which constitute the northern slopes of Gebel Haurân. Near Hît the soil is fertile; the district is inhabited and cultivated; being bare of trees, and monotonous in aspect, it exhibits the basaltic rock cropping out in places, and large black fragments and broken masses lying thinly on the surface. "In a *geological* point of view," says Mr Porter, "the Lejah is the most wonderful district I have ever seen." As viewed "from Hît," it "resembles a lake agitated by a strong wind; and any one who has seen Loch Lomond while a winter tempest swept over it, and the troubled waters assumed the gloomy hue of the clouded heavens, may form some idea of the appearance of the Lejah as seen from" there. From its eastern border stretches out a broad plain as far as the eye can see. Only a few of those singular conical mounds, or hills, called Tells, are visible within the Lejah; one, Tell Amâra, is about 300 feet high; another, Tell Sumeid, though not so lofty, is nearer to Shuhba, whilst Tell Shuhba itself is an extinct crater. Burckhardt has named the centre of this region "the Inner Lejah," where the black rocks are higher, and the ground itself is more broken.

Thence succeeds the Greek province of Auranitis to the west of Suweideh, and reaching as far as Bostra, its southern border may be taken nearly in a line parallel with Wadi Zêdy. The boundaries of these provinces appear to have varied at different times; and in fact to define them with geographical accuracy is, I think, a very difficult task.

The Haurân, so named both in Hebrew and in Arabic, was a portion of the ancient kingdom of Bashan, and in its general appearance is an immense and fertile plain, but broken by many Tells of a cone shape. The ground is considered as the most fertile in Syria, and is covered with cultivated fields and inhabited villages. Where any water is present, and after the rainy season, much of the plain is green with rich grass. Many ruined cities are also met with. They are built like those in the Lejah, of black basalt; and, as Mr Graham says, since "all the plain of the Haurân is destitute of trees, there is nothing to relieve the gloom of these black towns." And according to Mr Porter, the plain of the Haurân stretches out to the horizon; its soil is deep, black, and loamy, in general free from stones and gravel, though around the base of the little *Tells* lie fragments of porous lava accompanied with rock of a firmer texture.

On the east side of the Lejah and the Haurân there extends for a considerable distance nearly N. and S. the ancient province of Batanæa—which is only the Romanized form of the word Bashan—and now called by the natives Ard El Bathanyeh—*i.e.*, the district of Bathanyeh. It would seem to range from Salkhad on the south, to Tells Khaledyeh and Asfar on the north, its mean breadth being perhaps about twelve or thirteen miles. Here the beautiful range of hills, Gebel Haurân, extends its picturesque form for about thirty miles, the central plateau of which is, according to Russegger, 2500 Paris feet, or 2670 English feet above the sea. The same traveller estimates its highest point at 6400 English feet. Of this range two conical peaks are the most conspicuous—Abu Tumeis, or "Father Tumeis," on the north, near to Shuhba; and the second, El Kuleib, or "the Little Heart," above Kufr, raises its graceful and cordiform or cordate summit. They both present very prominent objects in the landscape. The eastern face of this latter cone is stated by

Mr Porter to be "naked, and of a dusty red colour, as if covered with a thick stratum of ashes ; on all the other sides it is clothed with oak forests. It is probably an old crater." He also says it is "the highest peak of Gebel Haurân." This, then, is worthy to be considered "as the Hill of Basan, (as) God's hill, even an high hill, as the Hill of Basan." Then Hebrân is conspicuous on the south, being "situated on the summit of a mountain ridge, which equals, if it does not surpass, in altitude any of the peaks south of the Kuleib." In this part of Bathanyeh, Mr Graham writes,* "one after another, the narrow green valleys opened before me, as I crossed the mountain chain; and here began the forests of oak which are so often mentioned in the sacred writings, but which now only exist in a small portion of Bashan." The "green valleys" here named, without doubt formerly constituted some of the rich pastures in which were fed the famous flocks and herds—the "rams, lambs, goats, bullocks, all of them," being called in Scripture "fatlings of Bashan."

Since there are no British species of oak found in that part of the Holy Land, and since Mr Graham expressly writes that "the acorns of the Bashan oaks are immensely large," there can, I think, be no doubt but the chief oak there growing is the "Valani Oak," or *Quercus Ægilops*, which receives that appellation *par excellence* of its large acorns—Valani being the Greek word Βαλανοι (B being pronounced V in modern Greek), signifying acorns. So Mr Graham adds, "There is likewise a general name for all oaks—Balûteh, or 'Acorn-Tree ;' Balût, meaning an 'acorn.'" From this Arabic word, then, the name of the evergreen species, *Quercus Ballota*, is clearly taken. The *Quercus Ilex*, called "Sindyân" in Arabic, is also there indigenous, together with, most likely, the *Quercus coccifera*, whose leaves are prickly and evergreen ; and perhaps the *Quercus Cerris* may be also present. But I apprehend the true species alluded to as the "Oak of Bashan" is the *Quercus Ægilops*, which grows to a goodly-sized tree, and which Isaiah names together with "the cedar of Lebanon, that is high and lifted up." This subject I have thought worthy of thus being somewhat dwelt on, because to all of us natives of Britain, "the oak" from the forest on the Kuleib (or the "Little Heart") of Bashan reminds us of our "Heart of Oak" indigenous in our own forests and hills.

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* Journal Royal Geographical Society, vol. xxviii. p. 245.

Mr Porter thus describes the general outline of Gebel Haurân,—from the north, "from Tell Abu Tumeis, the mountain chain preserves an almost uniform altitude" (which Russegger terms a *plateau*) "to Tell Kuleib. From this latter point it slopes down gradually to Salkhad. The whole of the loftier portion is clothed with oak forests, and the scenery is rich and varied, with bold precipices, deep ravines, and picturesque vales. South of the Kuleib the chain branches, or rather is divided by a wadi, which extends from near the eastern base of that peak to (about three miles) west of Salkhad. The eastern portion is, toward the south, destitute of trees, but is nearly all capable of cultivation in patches and terraces, where the loose stones are gathered off the soil." "The northern slopes are of a different character. There the gentle declivities are only diversified with little naked conical hills, but the extreme richness of the soil amply compensates for these defects."

I believe the entire range of Haurân is of the volcanic rock, though there may possibly be limestone in places associated with it, but which I think no one has yet detected. And probably it is to the volcanic action, which at an early period must have been so powerful and so general in this district, that the prophet Nahum alludes, when he says,—"Bashan languisheth." . . . "The mountains quake at him and the hills *melt*, and the earth is *burned* at his presence." . . . "His fury is poured out like fire, and the rocks are thrown down by him."

Burckhardt has called this volcanic rock, basalt, black-stone, black trap, and tufwacke. The previous traveller, Seetzen, entitles it basalt.* This black-stone being very heavy, many considered that much iron was contained in it; and at Salkhad, the ancient Salcah, Burckhardt writes, the Tell or "hill upon which the castle stands, consists of alternate layers of the common black tufwacke of the country, and of a very porous, deep-red, and often rose-coloured pumice-stone. In some caverns formed of the latter, saltpetre collects in great quantities." On a further examination of the remarkable Tells so frequent in this region, I am satisfied more will be proved to be of igneous origin. Mr Porter ascended one on the north-west side of Abu Tumeis, near Shuhba. He describes his "ascent, though not long," to have been "difficult and toilsome, owing to the deep coating of small black stones, intermixed with porous lava, and light tinkling cinders, with which the whole hill-side is covered." . . . "On the western side of the hill," he found a "large, deep, bowl-shaped cavity, with the jagged upheaved rocks forming a rim all round, and the whole exterior and interior coated with the debris of former eruptions." He says, he "had seen several craters before, but none so distinctly-marked as this."

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* Future investigations must determine what these rocks really are,—tuff, lava, scoriæ, trachyte, dolerite, trap, or other igneous products.

And again, on ascending to the castle at Salkhad, which is "situated on the summit of a conical hill, from 300 to 400 feet above the city," he saw that it had at one time been "the crater of a volcano," and that "its sides are still in many places covered with light cinders, and blocks of lava, similar to those on Tell Shuhba." "The whole country," he adds, from Salkhad, "as far as the eye can see, is volcanic. All the stones that are strewn over the soil are porous trap, and the rock that occasionally crops up is of the same kind." At the extremity of the province of Auranitis, the ancient city of Busrah, and the former capital thereof, is "situated in a plain," now deserted, but "of unrivalled fertility," as I have witnessed in many volcanic districts, such as the plains below Vesuvius and Etna; and it produced, from "the richness of its soil," not only an abundance of excellent corn, but also of oil and wine. On many of its ancient coins is seen a bunch of grapes, in allusion to the excellence of its wine, as well as to its name in Greek Βόστϱα, the word Βότϱυς signifying "a bunch of grapes;" and by a transposition of letters becomes Βόστϱυ or Bostra.

Now, "Bashan (has) shaken off its fruits." . . . "The vine is *dried* up;" . . . and "even all the trees of the field are withered." "Of the vineyards for which Bozrah was celebrated," and to whose "wine-press" allusion is made in Isaiah (lxiii. 3), Burckhardt distinctly records, "not a vestige remains;" and the very city itself, with its vast ruins, has "become a desolation, a reproach, (and) a waste."

Saltpetre is a mineral found in this part of the Haurân; but it is perhaps more abundant in the Lejah, both in the caverns of the black volcanic stone and in the soil from amongst the ruined houses. Among some of the Tells in the corner of the Lejah, near the western roots of the Haurân range, Burckhardt discovered not only much pumice-stone, but also porous tufa.

From a tower in Busrah, Mr Porter describes the country on the south and south-east as a plain, "extending to the horizon, unbroken by hill or mountain, and the soil" to be apparently, like that on the north, "rich and fertile." But the road from Busrah towards Salkhad gradually rises with a gentle ascent, and after about three miles, near Burd, the aspect of the country changes to a savage and bare appearance, with quantities of stones, and becomes broken and undulating, with rugged Tells here and there.

Beneath the Castle-hill of Salkhad, the eastern ridge of the Gebel Haurân, as Mr Porter says, "sinks down into a plain. Due south, there is a slight depression in the plain, with a gentle swell on each side, running as far as the eye can see, in a straight line." "To the south-east, an ancient road runs across the fine plain, passing the bases" of two Tells; it extends, according to the Arab guide, to the Euphrates and Bassora. The plain likewise stretched out far away to the east.

To the north and north-east the great plain, El Harrah, of which I have already spoken, extends. Mr Graham believes this strip of desert to be five days' journey in length, which, taking a day's journey at 30 miles, would make 150 miles to the east, and a day, or a day and a half's, or 30 to 45 miles, in breadth, from north to south. "The whole plain is covered with innumerable stones, rounded like boulders, but all of basalt;" and these lie "so closely together, that the dromedaries could hardly make their way across them." A continuation of the ancient road seen at Malah and Deir E' Nazrani led entirely through this strip, and is supposed to lead through the Great

Desert straight to Palmyra. The road was unpaved, and "all the stones had been carefully picked out and piled up against the side, so that a fine wide space was left for the road."

All the ruins and ancient buildings were composed of the same basaltic stone. And so were the "thousands of stones" bearing the rude figures of animals, and the numerous inscriptions in the unknown characters. All this district presented nothing but desolation,—no living creatures, no trees, and no shrubs. None of the four Tells, Nemareh, Ozda, Midneh, or Jerid, which Mr Graham saw, exhibited any traces of an extinct crater—at least he makes no mention of any such phenomenon; consequently, the craters in Gebel Haurân on the west, and most likely others in the range of the Safah on the north, must have been the centres of the former volcanic action over this extensive plain, and the sources from whence the innumerable stones of black basalt have been derived. Nor does he particularly state what the nature of the rock may be under the surface of El Harrah, whether volcanic, or limestone, or sandstone. I cannot, however, but think that a portion of this desert must exhibit—perhaps, indeed, in that part lying more to the east—traces of the limestones belonging to the *cretaceous* series, which, as I explained in my former Memoir on the Desert of Sinai, &c., which I read at the Meeting of the British Association at Birmingham in 1849, extended through El Nejd, a district of the Great Arabian Desert, to the north and east beyond Petra. Mr Graham only once describes a *white stone*, which most probably belongs to that series. The town called Kharbet El Beida, meaning "the *white* ruin," situated to the south-east of E' Safah, was built entirely of *white* stone.

He writes, "The region E' Safah, from which the chain rises, which is called Tellul e' Safah, has, I suppose, few parallels." This chain constitutes a range more than twenty miles long, "having nineteen distinct peaks; the highest of them may be about 600 feet above the level of the plain." It is a volcanic district, or rather a "volcanic island rising abruptly out of the desert. Its breadth is in some places 15 miles. The appearance it presents internally is most remarkable. If we imagine a vast quantity of molten metal to be confined in some vessel, and its surface violently agitated by some powerful agent, and, while in that state, suppose the mass suddenly to be cooled, then the appearance which the surface of the metal may be conceived to assume would probably be most nearly exemplified in the wild and savage aspect of the Safah. It resembles no other formation that I am aware of except the Lejah; and both these districts resemble more nearly the appearance presented to us by some of the volcanic regions in the moon than anything we have on our own globe." The *Safah,* continues Mr Graham, "is even more horrible than the Lejah, for there, at least, are many patches of good soil." It has "no springs, and the only water here is collected in hollows from the rains."

"The hills which rise from the Safah are at distances varying from four to ten miles from the edge; from the foot of these hills to the border, this sea of basalt is intersected with cracks and fissures, sometimes 20 feet in breadth, and many more in depth. The ravines are quite impassable, and frequently such a fissure has to be followed a very great distance before it becomes possible to cross it." So also, "in some places" in the Lejah, Mr Graham tells us, "the horses could hardly keep their footing; in others, we had to jump and scramble across large fissures in the basalt;" and "the description which he has thus given of the Safah will apply equally to the Lejah. They are two of the most remarkable instances of a volcanic formation perhaps in existence."

"The physical features of the Lejah," writes Mr Porter, in his work "On Damascus" (vol. ii. p. 241), "present the most singular phenomena I have ever witnessed, and to which there is not, so far as I know, a parallel in the world, with the exception of the Safah. It is wholly composed of black basalt rock, which appears to have in past ages issued from innumerable pores in the earth in a liquid state, and to have flowed out on every side until the plain was almost covered. Before cooling, its surface was agitated by some fearful tempest or other such agency; and it was afterwards shattered and rent by internal convulsions and vibrations. The cup-like cavities from which the liquid mass was projected are still seen, and likewise the wavy surface a thick liquid generally assumes which cools while flowing. There are in many places deep fissures and yawning gulfs, with rugged, broken edges, while in other places are jagged heaps of rock that seem not to have been sufficiently heated to flow, but were forced upwards by a mighty agency, and then rent and shattered to their centre. The rock is filled with little pits and protuberances like air-bubbles; it is as hard as flint, and emits a sharp metallic sound when struck. I did not observe any approach to columnar or crystallised basalt."

I am not aware that among the phenomena exhibited by the volcanic districts in the south of Europe, with which I am familiar, either around Naples and Vesuvius, or within the Isle of Ischia, or in the vicinity of the more glorious Etna, there are any of the like extent and magnitude, nor did I see there present any such deep chasms and fissures* as those observed in the Lejah and Safah; but if we turn to that entirely volcanic island in the north-west of Europe, and just within its geographical range—Iceland—we shall perceive the nearest parallel to some of those phenomena now under notice, especially to the enormous clefts or fissures, named in Icelandic *gias*. "Near Thingvalla," Sir George M'Kenzie has written, in his able "Travels in Iceland," "we entered a deep and frightful fissure called *Almannagiau*. This has been formed, with many others of smaller dimensions, and another large one which runs parallel to it at a considerable distance, by the sinking of the ground during some of those terrible convulsions which have shaken Iceland to its foundations. The whole rock bears marks of having been affected by fire. We came suddenly upon the brink of the precipice, and were turning aside from a scene so fearful, when we were told that we must descend."

Pliny Miles in 1852, just before arriving at Thingvalla, "while jogging along on the level ground, came suddenly upon the brink of an immense chasm, 150 feet deep, and about the same in breadth." This, he says, is "one of those seams or rents in the earth common in Iceland; originally a crack in a bed of lava. Its precipitous sides and immense depth seemed at once a bar to our progress; and without a bridge over it, or ropes or wings, we saw no way of getting along without going round it. Without seeing either end, and wondering how we were to get round it, we were told we must go through it. At length we made our way to the bottom. We were in a deep chasm or defile, the wall on the west side being over 100 feet high, and on a level with the country behind it. The wall on the east side was lower, and beyond this wall the country was on a level with the bottom where we were. This chasm is called the *Almannagja*, or 'All-men's-Cave.'" Again, the same traveller adds, in another district, "We travelled several miles over a most desolate volcanic region, completely covered with lava rocks, scoriæ, and volcanic sand. Like all the lava-covered country, it was broken up in huge, irregular masses, and very cavernous—in some places showing caves 30 or 40 feet deep. No description or picture will give a good idea of the old lava on the surface of the ground, to a person who has never been in a volcanic country. Not the roughest limestone region I have ever seen will bear the slightest comparison with the lava-covered districts (near two-thirds of the surface) of Iceland. In written descriptions of volcanic regions we often see mention made of 'streams of lava.' These streams in other countries are usually down the sides of mountains, but here in Iceland they extend for miles along the surface of the level ground, and we are puzzled to know where they came from, for usually we see no crater or mountain anywhere near. I have seen these 'streams' standing up in bold relief, a black, rough, horrid mass from 10 to 100 feet deep, several hundred yards wide, and one or two miles in length."

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* But in passing through Masca Lucia, between Nicolosi and Catania, on my descent from Etna, I learned that a large and deep fissure existed near that place. I do not know whether it be in a lava-bed or not.

Also in 1853, an amusing yacht-voyager writes, near the lake Thingvalla, "we found ourselves upon the brink of one of those extraordinary chasms which the natives call by the euphonious name of *gja*. And truly no name can be rugged or break-jawed enough to express the horror of one of these break-neck *gjas*. The craters in the moon are the only familiar objects to which the general reader may be referred, to catch a glimpse of their astounding reality. Or imagine two cinders, about a couple of miles long, placed a little way apart, and viewed through a "microscope of extra power." and you will have some notion of this *Hrafnagja*, or "Raven Chasm," on the brink of which our little ponies now stood, and across which our unflinching pathway lay." And describing Thingvalla, he continues: "It is in immediate proximity to the most wonderful of all the *gjas*, called *Almannagja*. This is a huge chasm some miles in length, stretching in a northerly direction from the head of the Thingvalla lake. It divides the great plain into two distinct parts, of which the westernmost is of a much higher level than the eastern one. And it would seem as if this yawning chasm had been made by the sudden upraising of the western portion of the plain by means of some subterranean agency. It should be remarked, that the western face of the chasm is precipitous, while the eastern and lower side has a gradual slope, as if it had fallen away from the other during the upheaving." And leaving that lake, and passing over a large lava-field, he "came upon a curious crater-like mound, composed of crisp cinder, of all manner of bright colours. On reaching it we found it was a mere shell, hollow within, and we could look down from the top into a spacious cavern, extending far on all sides. Indeed, we could not but come to the conclusion that the whole of this lava-field is more or less hollow; for many were the great caverns we passed as we rode along."

And in 1855, Mr Robert Chambers, in his pleasing "Tracings of Iceland," describes the Almannagiau as "but one of the lateral rents resulting from subsidence. Along the rising ground to the east appear four more, one above the other, and all parallel." Again, he mentions the Hrafnagiau, as "a terrific abyss, which extends for two or three miles, with a width of from ten to thirty feet, and would be totally impassable, but for a few blocks which have fallen in at a certain place, and to some extent filled up the gulf. From the general narrowness, irregularity, and darkness of this chasm, it would form an admirable retreat for robbers. The surface continues for miles to exhibit the same fractured character, with less appalling effects. There are also short minor subsidences, leaving a piece of surface forty feet or so below the rest, while, at the ends, a thin superficial crust having remained at the original level, 'antres vast' have been formed."

I need not, however, insert any further descriptions of awful fissures, chasms, rents, caverns, or basaltic and lava masses, which occur in other portions of Iceland, as parallels to those in the Lejah and Safah, that have been so well detailed by Mr Porter and Mr Graham.

Proceeding westwards from the Safah, "the group of conical hills called E' Tellul," as Mr Porter describes it, appears on the north, and "is about fifteen miles in length," running, like the Safah, near north and south. The Tell named Dukweh is the highest cone-shaped peak, and rises nearly in the centre of the Tellul. From its southern end to Gebel Haurân there extends a vast plain.

Lastly, on the plain beyond, to the west, is seen the southernmost of the three black ruins called E' Diûra—that is to say, "The Convents." It is a square tower, built of hard basalt, and is most likely a border fortress; and, in fact, Ammianus Marcellinus (lib. xiv.) mentions that the early inhabitants built castles and strong forts as defences against the incursions of the neighbouring hostile tribes.

And, in concluding this imperfect communication, I must express an earnest hope that some enterprising British geologist would undertake a scientific journey throughout the mountains of Haurân, and the volcanic districts in the neighbouring Harrah, the Tellul, and the Safah, in order to determine with more exactness the remarkable natural phenomena and different formations which there present themselves.

On the Application of certain Laws of Heat and Combustion to the Use and Economy of Fuel. Being the substance of a Paper read before the Aberdeen Philosophical Society, 11*th February* 1859. By ALEXANDER D. MILNE. Part II.

Of the Hydrogen Element in ordinary Fuel, and the influence it exercises on Combustion.

Common coal generally contains about 80 per cent. of carbon, and only 3 to 5 per cent. of hydrogen. While, therefore, we are justified in applying generally the laws which regulate the combustion of carbon to the use of common coal, we cannot overlook the influence of hydrogen, as it modifies greatly the action of the other element. This will be readily granted, because, 1*st*, Its heating power, weight for weight, is three to four times that of carbon; 2*d*, To its presence we owe the production of illuminating gas when coal is distilled; 3*d*, It is also solely and entirely the origin of flame, and smoke or soot, in the combustion of coal. These are all most important considerations.

In Table II. we give the particulars of its combustion. We would draw special attention to column 9, in which we deduct 8685° Fahr. from its heating power when used for ordinary purposes, such as raising of steam, and, indeed, for all purposes where the terminal temperature exceeds 212°, because above 212° that amount of caloric (8685°) is latent, and for all heating and pyrometrical ends may be regarded as non-existent. If we cool the products below 212° this caloric becomes sensible; and this, Andrews and other experimenters did. In several treatises on fuel this allowance is not made; the heating power is assumed, for practical purposes, as 60,840°, and the temperature upwards of 4000° Fahrenheit, while in reality they are only 52,155° and 3476° respectively.

We might have given for hydrogen a table precisely similar to Table III., but it is unnecessary. The same laws apply to both elements, with this difference: In carbon, the original products of combustion (carbonic acid and nitrogen) have ·2596 specific heat; and all successive additions of air in excess have ·2669 specific heat—almost the same in both cases. But in burning hydrogen, the original products (vapour of water and nitrogen) have ·4191 specific heat, which is much higher than that of air. This necessitates a change in the formulæ for hydrogen, to suit *diminishing specific heat* with every addition of draught. We shall simply give formulæ, assuming 52,155° as the heating power, and 3476° (in round numbers 3500°) as the initial temperature with 1 equivalent of draught.

Formula 1. In the combustion of hydrogen, to find the initial temperature due to any amount of air or draught, divide 3500° by 1 for 1 equivalent of draught. For every additional equivalent, or part of equivalent, increase the divisor by ·6, or a proportional part of ·6. Examples—

With 1· draught the initial temperature is 3500° ÷ 1· = 3500°
... 1·4 do. do. 3500° ÷ 1·24 = 2822°
... 2· do. do. 3500° ÷ 1·6 = 2187°
... 2.5 do. do. 3500° ÷ 1·9 = 1842°

Where both carbon and hydrogen are present, we must use both formulæ, if the proportions are ascertainable in which the two are present.

Formula 2. To find the loss of heat by waste products, multiply the terminal temperature by the equivalents of draught, one equivalent being represented by 1·, two by 1·6, three by 2·2, &c., as in last formula. The constant number, 3500°, is to the product as the whole caloric capable of being utilised is to the loss. Examples—

Draught 1· Ter. temp. 500° Then 500° × 1· = Loss 500°=14·3 per ct. of 3500°
... 2· ... 300° ... 300° × 1·6 = ... 480°=13·7 ...
... 1·2 ... 1000° ... 1000° × 1·12= ... 1120°=32·0 ...

Formula 3. To find the proportion of caloric which is non-effective, so far as the hydrogen element is concerned, proceed as in formula 2; for the actual terminal temperature substituting the temperature of the surface or body to which the heat is applied.

In applying these rules, we require an approximation as to the quantities of carbon and hydrogen present, in order to estimate the combined influence.

It will be seen that with 2 draught the loss of heat is about equal in carbon and hydrogen for the same terminal temperature. With less draught, the loss is greater in hydrogen than in carbon; with more draught, it is rather less.

Absorbing Surface required in the case of Hydrogen.

The current will be subject to the same laws as that from carbon—viz., as the difference of temperature between the absorbing surface and the current increases, the quantity of caloric communicated in the same space and time will increase also, and *vice versa*. But the actual velocity of communication at the same temperature is different for different gases. (See Gmelin, vol. i. p. 233). Until, therefore, this point has been determined with regard to each of the two distinct currents we have had under consideration, no definite comparison can be instituted between the heat-giving powers under any given conditions of temperature or draught. But if we assume the power of giving out heat as equal in carbon and hydrogen at same temperatures, then it will follow that carbon, producing with the same draught a much higher temperature than hydrogen, will be more economical, and require less boiler space for absorption.

Temp. of current from Carbon.	Draught 1 — Temp.	4400°	2 — 2200°	3 — 1466°	4 — 1100°
... Hydrogen.	... 1 ...	3500°	2 — 2187°	3 — 1591°	4 — 1250°

In excessive draughts—three or four equivalents or upwards, the temperatures are nearly equal—hydrogen slightly in excess; but the difference in favour of carbon is very marked when the draught is one to two equivalents. And perhaps the fact that bituminous or hydrogenous fuels do not in practice approach so closely to their theoretical power of heating as do carbonaceous fuels, may be partly owing to the operation of this law; although there are also better known causes at work producing such a result.

On the Joint-Combustion of Carbon and Hydrogen, as they exist in ordinary Coal.

For details as to composition, modes of analysis, &c., we must refer to the proper authorities, and shall still endeavour to apply ascertained first principles to the phenomena of combustion.

The ordinary constituents of coal are,—carbon; hydrogen, combined with oxygen, as water; hydrogen, not combined with oxygen; and ashes or mineral matter.

The table below is founded on the elaborate investigations by Dr L. Playfair and Sir H. de la Beche. Columns 8 to 12 we add as illustrative of future remarks.

Locality.	Average Composition, 100 parts.					On Distillation, there is						Proportion of Hydrogen to Carbon.
						Left as Coke or Cinder.		Expelled in Gaseous Form.				
									As Volatile Hydrocarbons.			
	Carbon.	Hydrogen.	Water, Hydrogen and Oxygen.	Sulphur and Nitrogen.	Incombustible Ash.	Ash.	Carbon.	Water, Sulphur and Nitrogen.	Hydrog.	Carbon.	Total.	
Wales.........	83·78	4·27	4·67	2·41	4·91	4·91	67·69	6·68	4·27	16·09	20·36	1 to 3·8
Newcastle...	82·12	4·60	6·40	2·59	3·77	3·77	56·90	8·99	4·60	25·22	29·82	1 to 5·5
Lancashire..	77·90	4·53	10·72	2·74	4·88	4·88	55·34	13·46	4·53	22·56	26·69	1 to 5·4
Scotland.....	78·53	4·40	10·90	2·11	4·03	4·03	50·19	13·01	4·40	28·34	32·74	1 to 6·4
Derbyshire.	79·68	3·66	11·56	2·42	2·65	2·65	56·67	13·98	3·66	23 01	26·67	1 to 6·3
	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.

Pure carbon, on distillation, yields nothing volatile.

Coal or wood, as the table shows, evolves a variable amount of gaseous matter, containing the volatile bodies in its composition, with the addition of 16 to 23 per cent. of carbon rendered gaseous and volatile by combination with the free hydrogen.

The hydrogen and carbon in combination may be evolved as tarry vapours, light-carburretted hydrogen, olefiant gas, or in various other forms of hydrocarbon. Much depends on the temperature employed—if it is low, tarry vapours will be largely formed; as the temperature rises, the proportion of carbon, as compared with hydrogen, diminishes. Speaking generally, 1*st*, At a low temperature, 200° to 600°, water is expelled; 2*d*, At 600° and upwards, various liquid hydrocarbons, tar, &c., with a large proportion of carbon; 3*d*, As the temperature rises, hydrocarbons, with still less carbon, as olefiant (1 hydrogen to 6 carbon); 4*th*, Hydrocarbons with still less carbon, as light-carburretted hydrogen (1 hydrogen to 3 carbon); 5*th*, At intense heat, a minute quantity of pure hydrogen.

Precisely similar are the changes when fresh fuel is thrown on ignited fuel, only the whole process is much more rapid, and combustion is at the same time going on in the part already evolved.

Taking Newcastle coal as an example, suppose 100 lb. thrown into a furnace containing brightly ignited cinder or coke, at intervals of 10 minutes, the phenomena that follow may be roughly stated thus:—

6·40	lb.	Water expelled at the very outset.
29·82	„	Volatile hydrocarbons expelled, commencing with those containing much carbon, and ending with almost pure hydrogen.
56·90	„	Fixed carbon or cinder, which has now reached the temperature of ignition, commences to burn; while during the whole period the carbon or cinder previously in the furnace has been burning steadily.
3·77	„	Ash or clinker remains as incombustible.
2·69	„	Nitrogen and sulphur, the period of evolution of which is immaterial for our present purpose.
99·58	lb.	Coal.

Generally the volatile ingredients are expelled in the first-fourth, or first-third of the interval, although not necessarily so.

With these data before us, we would remark—

1. In ordinary bituminous coal, yielding a gaseous combustible, the process of combustion is distinctly divisible into two parts, as regards the *locality* of its occurrence. *First*, The fixed carbon or cinder burns on the hearth more or less uniformly and continuously. *Secondly*, The volatile part must burn, if burned at all, in the *body of the furnace*, or space *above the fuel*, into which space it is projected as evolved.

With Newcastle coal let us estimate the air required for these separate processes of combustion—

				Air.
Fixed carbon or cinder burning on the hearth,	56·9 lb.	. . . .	× 11·6 lb. air =	660 lb.
Volatile hydrocarbons burning in the body of the furnace,	29·82 lb. consisting of	Hydrogen, 4·6 lb. Carbon, 25·22 lb.	× 34·8 lb. air = 161 × 11·6 lb. air = 292	453 lb.
		Total atmospheric air required.	. . .	1113 lb.

For complete combustion, therefore, 660 lb. air must be brought into contact with the cinder on the hearth, and 453 lb. into contact with the hydrocarbon gases above the hearth.

2. In the case supposed, perfection, so far as attainable, would evidently be, first, a uniform rate of combustion of the cinder on the hearth, at the rate of 5·69 lb. per minute under the influence of a constant current of 66 lb. air per minute; and secondly, a uniform evolution of gaseous combustible in the furnace at the rate of 2·98 lb. per minute, and its combustion under the influence of a constant supply of 45·3 lb. air per minute. And, with regard to the proper points of

admission of the two currents of air, several reasons might be given why, the former supply being admitted as usual through the bars, the latter should be admitted by an opening above the bars, so placed as that the gas, as evolved, shall be projected into an atmosphere containing air sufficient for its combustion. The most important of these reasons we would express thus:—

"Combustion of a hydrocarbon may be accomplished either by introducing it into an atmosphere containing common air, or by introducing common air into an atmosphere containing the hydrocarbon; but in the former case the combustion is much more complete than in the latter."

Hydrocarbon projected into air.—A gas jet affords a familiar illustration of this. The combustion is almost perfect—no smoke is produced, but intense light and heat. Outside the flame the hydrogen burns producing an atmosphere of 3500° temperature; and through this barrier the carbon must pass before it escape. The heat is so intense, however, that it becomes white hot, and burns into carbonic acid with an accompanying temperature of 4400°. The heat thus kept up continues the process, decomposing the gas into hydrogen and white hot carbon, which again burn in succession.

Air projected into an atmosphere of hydrocarbon.—Such a jet will burn if lighted; but the results are very different. Gmelin thus describes the appearance:—"The flame appears to be dark within; then a brilliant envelope, hot enough to melt platinum; then toward the outside a dark yellow flame, lengthening above, and containing soot, the greater part of which remains unburnt." Why this difference? The jet of air is surrounded by an atmosphere of hydrocarbon. The hydrogen first takes fire with a heat of 3500° as the consequence. This heat is communicated to the adjacent hydrocarbon, decomposing it into carbon and hydrogen. But the carbon is simply heated, and rising with the ascending current quickly cools from white hot to yellow, from yellow to red, from red to black. It has now become soot. In short, when we burn a jet of hydrocarbon in air, the carbon has to pass through a barrier of flame ensuring its combustion;

when we burn hydrocarbon around a jet of air, the hydrogen burns, but the carbon is simply heated, and in time deposits as soot.

To apply this to the case before us. By introducing above the fuel the air required for the gaseous portion, we burn the hydrocarbon in an atmosphere of air; while by drawing this air through the fuel, we to a great extent burn the hydrocarbon by jets of air.

Any one may satisfy himself that the latter is the case generally in practice. The flame described by Gmelin—white, yellow, red, and finally black—is a common phenomenon in most furnaces. And it may be accepted as an axiom that *a flame which exhibits decreasing intensity of light, invariably ends in smoke.* A flame in which the carbon is being converted into carbonic acid, can never exhibit a red appearance; when that occurs, the temperature has already fallen below the point of ignition.

To return; we thus find that of the air required for Newcastle coal, 60 per cent. (660 lb.) should pass through the bars, and 40 per cent. (453 lb.) should be introduced above. And it appears to be most in accordance with the principle here involved, that if excess of air above the quantity chemically required be given, it should be above entirely. It is probable that a slight excess of air contributes to the combustion of the carbonaceous portion of hydrocarbon gases.

Assuming the regulation of draught as practicable, how may we best obtain that regular and uniform combustion of both fixed and volatile ingredients which can alone agree with a regular and uniform supply of air above and below the bars?

An automaton feeding apparatus, by which the fuel is constantly projected into the furnace in small quantities, seems most suitable. But as such apparatus requires a uniform motive power, and is inapplicable to the greater number of boiler furnaces, we shall confine our remarks to hand-fueling, and enumerate the points which tend to secure regular and uniform combustion, keeping previous remarks on minimum draught still in view.

1. Frequent fueling or firing.
2. Maintaining a uniform thickness or depth at all times,

and in all parts of the furnace. The effect of this is to render the resistance to the passage of the air uniform.

3. Maintaining considerable depth of fuel, much greater than is commonly the case. By no other means can we diminish the draught and raise the initial temperature,—consume the gaseous carbon and economise fuel.

4. Another most important end served by having great depth of fuel, and at the same time sufficient area of bar surface, is, that the draught being diminished, fewer passages are required for the hot gaseous products rising from the cinder below. The upper layers of fresh fuel may thus lie close without prejudice to the draught, are subjected to a more gradual heat, and consequently give off the gaseous combustible vapours more slowly and uniformly.

Influence of Excessive Draught on the joint Combustion of Carbon and Hydrogen.

In applying the principles previously laid down to this part of our inquiry, we must keep in view the nature and succession of the phenomena which accompany the combustion of hydrocarbons.

1. Previous to combustion, they are decomposed, under the influence of heat, into their elements, hydrogen and carbon. Flame is simply carbon precipitated in a visible form, and raised to a high temperature by the surrounding caloric.

2. The hydrogen element burns first. A very low temperature is sufficient; at 600°, considerably under red heat, it commences. The carbon is necessarily freed from combination, and deposited at the same moment, and its colour will indicate the temperature present.

3. If the temperature produced by the combustion of the hydrogen is high, or if caloric from any other source is present, the precipitated carbon burns also. This does not occur, however, until the temperature reaches a certain intensity. At a red heat, 800° to 900°, combustion of carbon slowly commences, but does not go on rapidly, until a much higher temperature, at least a white heat, is attained. If this temperature is not attained, the carbon passes off unconsumed, becoming red, dull red, and black, as it cools.

It appears evident, that in any ordinary furnace, the temperature may, and does, suffice to decompose the hydrocarbon, and burn the hydrogen; but whether the carbon thus separated be also burned, depends on the presence of a temperature of considerably greater intensity. And this temperature must be present, not *on the hearth,* but *in the open space above* in the body of the furnace, where the decomposition of the hydrocarbons takes place. In other words, the initial temperature must be high; and this can be effected only by the regulation of draught, as before indicated, and by presenting the fuel to its influence under proper conditions.

The colour of the flame or precipitated carbon affords an indication of the temperature at any point of a furnace. For a few data on this point, see Table I, columns A and B. The occurrence of black or dark red in the flame will thus indicate excessive draught. The truth of this indication may be relied on, as the results were verified by actual calculation of the amount of air passing. The only exception occurs where close proximity of a metallic conducting medium, such as boiler plate, cools the flame prematurely, even with limited draught. Such furnaces are, it is feared, incurably smoky in most cases.

We would recommend frequent attention to these appearances; and it will be found, with the above exception, that as the draught is reduced, and other arrangements altered to correspond, the flame will become reduced in size, and increased in brilliancy.

Since combustion of the gaseous carbon, as precipitated, is the same thing as prevention or consumption of smoke, we may recapitulate the principal points involved. *First,* Proper regulation of draught, by maintaining a high temperature in the body of the furnace, renders the combustion of the carbonaceous particles therein suspended possible. *Secondly,* It diminishes the rush of hot air through the fuel, and thus tends to produce regular and uniform evolution of the gas. *Thirdly,* It lengthens the period during which the carbonaceous particles are subjected to the undiminished initial temperature.

As a corollary, we may safely add, that every influence tending to diminish initial temperature tends to produce smoke and waste fuel. Damp in the fuel has a very cooling effect, as it absorbs both latent and sensible heat at the moment when it can be least spared.

But that which exerts the most powerful influence in reducing initial temperature is the practice, now so common, of completely enclosing the furnace in the boiler flue. The body of hot gaseous matter which it is desirable to maintain for a time, at a temperature of 2000° and upwards, in order that it may burn the carbonaceous particles suspended in it, is thus surrounded by a rapidly absorbing metallic surface, at a temperature of 250° to 300°. The consequence is, a rapid and premature diminution of the initial temperature, while the combustion of the precipitated carbon is suddenly arrested. Other things being equal, such furnaces produce more smoke than others, with every precaution as to fueling, and regulation of draught. This method has been highly recommended for this reason, that the furnace being limited by the boiler, its dimensions are not left to the discretion of parties considered incompetent. But a large furnace is not in itself an evil, as the experience of the Cornish miners for generations proves; and only becomes so when a system of thin fires, with excessive draught, is persisted in. Cornish practice, the most economical and successful in the world, is "thick fires, extensive grate area, and slow draught." The consequence must be, high initial temperature, gradual and uniform combustion, prevention of smoke, and the utmost economy of heat, both in its production and application.

Unless the furnace in which the heat is produced be removed from the boiler or other body to which the heat is to be applied, either entirely, or so far at least as to allow of a high initial temperature, we do not hope to see smoke generally consumed. In modern boiler-making, it seems to be forgot, that the fuel, both in its fixed and volatile constituents, is *consumed by caloric* while *generating* it. A style of furnace, which seems somewhat in accordance with the views here expressed, is mentioned in "Fuel and its Applications," by Drs Ronalds and Richardson, p. 264. It is there stated to be smoke-consuming.

Supposing a regulated draught, and gradual and uniform combustion of both fixed and gaseous combustibles attained, it may be useful to inquire what appearance the waste products ought to present, as they issue from the chimney-top. Assume 100 lb. coal burned in ten minutes = 10 lb. per minute; the product per minute should consist of—

30·1 lb. carbonic acid, invisible.
102·0 lb. nitrogen, do.
4·7 lb. vapour of water, visible.

This emission of steam, at the rate of 4 to 5 lb. per minute, will appear as a faint cloud, on contact with the cold atmosphere.

The ordinary appearance is very different. For the first two or three minutes after fueling, we see a dense black cloud of soot mingled with vapour of water, which suddenly ceases, leaving nothing but invisible carbonic acid and nitrogen, until another fueling occurs, to be followed by the same results.

As these sudden evolutions are always smoky, let us see what takes place in the furnace, supposing the gaseous evolution over in the first three minutes out of ten, with 100 lb. of fuel. The draught remains nearly uniform generally, whatever be the fluctuations in the combustion.

	First Minute.	Second Minute.	Third Minute.	Fourth Minute.	Fifth Minute.	Sixth Minute.	Seventh Minute.	Eighth Minute.	Ninth Minute.	Tenth Minute.	Air.
Cinder burning at uniform rate requires, ...	66	66	66	66	66	66	66	66	66	66	=660 lb.
Hydrocarbon, evolved in three minutes, requires, .	151	151	151	0	0	0	0	0	0	0	=453 lb.
Total requirements each minute,	217	217	217	66	66	66	66	66	66	66	=1113 lb.
With one draught, the *constant* current would be, . .	111	111	111	111	111	111	111	111	111	111	=1110 lb.
But to meet the requirements of the first three minutes, the constant current must be,	217	217	217	217	217	217	217	217	217	217	=2170 lb.

This system of fueling thus places us in a dilemma. If the draught is so adjusted as to be equal to one equivalent on the whole quantity of fuel, one-half of the combustibles escape unconsumed during the first three minutes. If air is supplied sufficient for the first period, it is in excess for the remainder, and amounts in all to two equivalents. This causes loss of heat, as before shown. Besides, the probability is, that after all, smoke will be produced, as there is more hydrocarbon present at one moment than when uniform combustion goes on, which involves a lower initial temperature; and the damp and water of composition has also a stronger influence, being more quickly expelled, along with the hydrocarbon. This explains why the admission of air above the fuel frequently fails in preventing smoke, and generally entails loss of fuel. The only remedy is uniform and constant evolution of gas and combustion of cinder; with uniform draught, and the other conditions already referred to.

Where uniform evolution and combustion cannot be attained, probably the next best plan is that proposed by Prideaux,—to admit a variable quantity of air *above*, to suit the variable evolution of gaseous matter.

In closing our remarks, it is evident that we have left untouched two very important heads of inquiry. *First*, We have carefully abstained from entering upon those practical details as to construction and size of furnace, &c., which the principles laid down have suggested. This has been done, from a conviction that much more depends on the regulation of draught, on depth of fuel, and especially on the position of the furnace, relative to the absorbing surface, &c., than on mere points of construction. For, however complete our arrangements may otherwise be, fuel will be wasted where draught is excessive, and smoke will be produced, more or less, where the flame, before it becomes converted into invisible gaseous matter, is allowed to touch a boiler-plate or other metallic conductor. In the *second* place, although we have assumed draught as capable of being calculated, and found it to be so, we have not entered on the subject. But the pneumatic laws which regulate the motion of gaseous currents are as definite as those which concern the phenomena of combustion, or the performance of the steam-engine. To those who object to the use of thermometers or pyrometers for initial and terminal temperatures, scales for registering area of damper, and gauges for force of draught, we would say,—look to the boiler, for the working of which the furnace is constructed, and the fuel consumed. Probably it is supplied with one or more gauges to indicate pressure, with gauge or float, to indicate depth of water, with safety-valve and fusible plug to prevent explosion, with atmospheric valve to prevent collapse. Why all these? They are required. The same may be said with equal truth of those appliances in connection with the furnace, to which we refer. If the fireman has to watch the pressure of steam, and the depth of water, why not the area of the damper, the force of draught, and the weight of fuel. Towards such a system of operation, the present paper may be, perhaps, received by those to whom the subject is important, as a slight contribution.

Notes on Californian Trees. By ANDREW MURRAY, F.R.S.E. PART II. (Plates VI., VII., VIII., IX.)

WELLINGTONIA GIGANTEA. The Mammoth Tree. (Woodcut, and Plates VI. and VII.)

The history of this long-lived tree has been so fully detailed by the various authors who have noticed it, and more particularly by Dr Seemann, so recently as March last, in the "Annals and Magazine of Natural History," that I should not have thought of including it as one of the subjects of my notes, were it not for the sake of some photographs of the tree sent me by my brother, copies of which will, I feel sure, be acceptable to the public.

It is so far well that the possession of these photographs should have in a measure constrained me to include a notice of this tree in my list, as most certainly notes on Californian trees, without any notice of the *Wellingtonia*, would have been as bad as "Hamlet with the part of Hamlet omitted." But I have little new to say regarding it, and I only offer, as a pendant to the sketches I have given, a short *resumé* of what has been already published by others, and most of which has been collected by Dr Seemann, to whom I offer my acknowledgments for the use I have made of his able paper.

The tree is said to have been first seen by the unfortunate Douglas in his Californian explorations; but this has now been shown to be a mistake. The route by which he travelled is perfectly known, and he never came within a hundred and twenty miles of any of the known examples of *Wellingtonia*. What he saw was *Sequoia sempervirens*, as may be otherwise inferred from the terms in which he speaks of it. The real *Wellingtonia* was first discovered by Mr Lobb, and introduced into this country in 1853, and described by Dr Lindley in the "Gardener's Chronicle" in that year. An ancient Californian tradition, of nearly ten years' existence, ascribes its discovery to a Mr J. M. Wooster, as one of the trees in the Mammoth Grove bears on its bark the inscription of "J. M. Wooster, Ju. 1850." Of course it had been discovered in a literal sense long previously by the Californian aborigines; but as priority of discovery depends upon priority of publication, they must give way; and as Mr Wooster's publication, at the best, can only be looked upon *as a manuscript notice*, we must, under the rules which regulate priority in such matters, hail Mr Lobb as first discoverer, although admittedly he himself was directed to it by general rumour current among the European settlers.

Dr Lindley is still more undeniably the first describer, and the name given by him to the tree (*Wellingtonia gigantea*) has of course precedence over all others. Notwithstanding this, the Americans made a strong effort to change the name into one bearing reference to Washington. As Dr Seemann tells us, "they even commenced in their newspapers an agitation against the adoption of the name *Wellingtonia*, quite ignoring that the savans of their country bow to the same code of scientific laws which govern the conduct of their European brethren, and that no amount of popular clamour could cause the right of priority here at stake to be set aside. When, therefore," says he, "Dr Winslow exhorted his countrymen in grandiloquent language to call the mammoth tree, if it be a *Taxodium*, *T. Washingtonium;* if a new genus, *Washingtonia Californica;* he simply proclaimed to all the world that he knew nothing whatever of the laws governing systematic botany." Perhaps the reader may like to see a specimen of the style under cover of which Dr Winslow proposed to effect this act of appropriation. It reads more like a speech concocted by Dickens for Mr Jefferson Brick than a real true *bona fide* speech. But I beg to assure them the article is genuine. It is as follows:—

"The name that has been applied to this tree by Professor Lindley, an English botanist, is *Wellingtonia gigantea.* By him it is declared to be so much unlike other coniferæ, as not only to be a new species, but to require description as a new genus. Other botanists of eminence think differently. To this, however, he has seen fit to apply the name of an English hero, a step indicating as much personal arrogance or weakness as scientific indelicacy; for it must have been a prominent idea in the mind of that person that American naturalists would regard with surprise and reluctance the application of a British name, however meritoriously honoured, when a name so worthy of immortal honour and renown as that of Washington would strike the mind of the world as far more suitable to the most gigantic and remarkable vegetable wonder indigenous to a country where his name is the most distinguished ornament. As he and his generation declared themselves independent of all English rule and political dictation, so American naturalists must in this case express their respectful dissent from all British scientific stamp acts. If the big tree be a *Taxodium*, let it be called now and for ever *Taxodium Washingtonium.* If it should be properly ranked as a new genus, then let it be called to the end of time *Washingtonia Californica.* The generic name indicates unparalleled greatness and grandeur; its specific name, the only locality in the world where it is found. No names can be more appropriate; and if it be in accordance with the views of American botanists, I trust the scientific honour of our country may be vindicated from foreign indelicacy by boldly discarding the name now applied to it, and by affixing to it that of the immortal man whose memory we all love and honour, and teach our children to adore. Under any and all circumstances, however, whether of perpetuity or extinction, the name of Wellington should be discarded, and that of Washington attached to it and transmitted to the schools of future ages."

Does the reader concur with Dr Seemann in thinking that all that this gentleman, who is so sensitively alive to the feelings of delicacy, shows in this oration, is ignorance of the laws governing systematic botany? With great deference, it seems to me to show an ignorance of something much more important—viz., ignorance of the first principles of common honesty. The appropriation in this instance would have been a double theft, first of the honour or right to his own appellation, which belongs to every sponsor; and next of the happy idea which led Dr Lindley to consecrate this grandest of trees to the grandest of our national heroes. As Messrs Sang and Co., nurserymen, Kirkcaldy, say, in an exceedingly neat and comprehensive account which they have published of the tree and its history, if the Americans want such a memorial for *their* great man, let them discover and describe their big trees for themselves. This attempt at appropriation, however, has failed. The better class of American botanists have repudiated it, and in a few years the name *Washingtonia* will have passed from the memories of men, except as a scientific, or rather *unscientific*, synonym.

Doubts, however, have been cast upon the distinctness of *Wellingtonia* as a genus, which, if well founded, might deprive us of that name. True, Dr Seemann, who is next in priority, has attempted to save it by condemning the specific name *gigantea*, as already preoccupied, and substituting *Sequoia Wellingtonia* for *Wellingtonia gigantea;* and his reasons for holding that the specific name *gigantea* has been already misapplied by Endlicher seem probable enough; but I trust we shall not require to settle this point. The genus seems perfectly good—as good, indeed, as any genus in this difficult and closely allied group of trees. The authors who have maintained that *Wellingtonia* and *Sequoia* are not generically distinct are Dr Seemann, Dr Torrey, and M. Decaisne. I am not aware that any other botanists have adhered to their views; but they speak with confidence, and would seem to imply that if Dr Lindley, at the time he proposed the genus, had had the materials which we now possess—(the male catkins are, I believe, what is referred to)—he would not have erected the tree into a separate genus; and hence, that we may infer that he now abandons it. They do not say this in so many words, but it is what one would naturally infer from Dr Seemann's expressions. On such a point as the soundness of the genus, it becomes me, a mere amateur in botany, to speak with great diffidence; but there is nothing which I have learned with more certainty from my zoological studies than that, in determining what elements are to be considered of generic value, no one set of characters can be wholly relied on. It is a just appreciation and balancing of the whole which leads the naturalist to a right conclusion. If he rests his views entirely upon one class of structure (whether it be the reproductive, the digestive, the respiratory, the vascular, the nervous, or the osseous systems) to the exclusion of the others, he will fall into error. And it must be the same with the botanist: if he builds his genera solely upon the reproductive organs, neglecting the respiratory (I mean the foliage), as I think has been done by Dr Seemann and Dr Torrey in this instance, I should anticipate that he must fall into error. In *Wellingtonia gigantea* and *Sequoia sempervirens* the difference in the foliage is most marked. Were there no other character to distinguish them, I should hold this to be sufficient. But with a just distrust of my own opinion in such a matter, I have applied to Dr Lindley himself to know whether any change has taken place in his views in consequence of the light thrown upon the subject by the additional materials, and the additional views (whose value I am far from depreciating) thrown out by the gentlemen I have named; and Dr Lindley has had the kindness to inform me, and allows me to inform the reader, that he has seen no reason to change his opinion. His letter is as follows:—

"Acton Green, Turnham Green, London, W.,
"5th January 1860.

"MY DEAR SIR,—Notwithstanding the criticisms of M. Decaisne, Dr Torrey, and Dr Seemann, I adhere to my opinion that *Wellingtonia* is necessarily distinguished from *Sequoia*, unless all the modern dismemberments of the old genera, *Pinus*, *Cupressus*, and *Thuja*, are to be cancelled—a measure in which I should not concur. It has not a little surprised me to find gentlemen who have no objection to offer to *Abies* as distinguished from *Pinus*, *Sequoia* itself from *Taxodium*, *Selaginella* from *Lycopodium*, *Lastrea* from *Aspidium*, *Leskea* and *Neckera* from *Hypnum*, and so on, nevertheless opposing the establishment of *Wellingtonia*. Surely systematical naturalists must allow, that, as structure becomes simpler and simpler, so must distinctive characters be sought in smaller and smaller differences. To apply the method of classification suitable for *Rosaceæ* to such an order as the coniferous seems to me unphilosophical.

"I therefore presume to differ from the authorities just mentioned; in doing which I am rejoiced to find that you agree with me.—Very truly yours,

"JOHN LINDLEY."

The difference is no doubt not great; chiefly, as above mentioned, in the character of the foliage. The differences in any of the other characters might, I think, in themselves be viewed as only of specific value: the cones, for instance, in *Sequoia* are smaller and more rounded; the male catkins also are more rounded and expanded; but the foliage is the true distinction; and I think we may be very glad to get such a distinction, to break up the tribe of *Cupressus*, which is so difficult and puzzling to distinguish and classify.

Like a great many of the North-West American trees, the *Wellingtonia* seems to be confined to isolated patches. Indeed it is a curious fact (as pointed out by Alphonse Decandolle) that trees, as distinguished from other plants, generally have confined ranges.

The first place where it was found was at a spot called the Calaveros Grove (more recently the Mammoth-Tree Grove), near the head-waters of the Stanislaus and San Antonio rivers, in long. 120° 10′ W., lat. 38° N., and about 4590 feet above the sea level. There the number of trees still standing amounts to 92. Two other localities are now known, one in Mariposa, and the other in Fresno county. The Mariposa grove contains about 400 trees, and the Fresno grove about 600; and it is from the former that the photographs which have furnished the accompanying plates have been taken. The tree is also said to have been met with in Carson Creek, a few miles to the north of Mammoth-Tree Grove; and Carrières stated that an officer of the French navy brought cones identical with those obtained in California from a latitude about ten degrees north of these localities, but the identity of these cones with those of the *Wellingtonia* has been doubted. It is said also to have been met with in various other parts of the Sierra Nevada; but if so, it does not there attain the gigantic dimensions of those in the groves above mentioned.

The tree is undoubtedly the largest and most magnificent known on the face of the earth. Its ally, the *Sequoia sempervirens*, is not far short of it in size, but still stands a little in the background. The average dimensions of both trees when full grown are about 300 feet in height and 90 feet in circumference. We have great difficulty in realising this immense height, and to assist us we must have recourse to other objects of comparison. To an Edinburgh man we have a very good one. The Gas Company's great chimney, although built in a hollow deep below Nelson's Monument, yet has its top 7 feet higher. Now it is only 329 feet high in all, including its pedestal, which is 65 feet in height; and as we shall presently see, one of these mammoth trees was actually 450 feet high, or nearly a third higher than that tremendous chimney. And Lord Richard Grosvenor, in a recent number of the "Gardeners' Chronicle" (7th January 1860), speaks of one he had just seen as 116 feet in circumference, and 450 feet high. It is taller than St Peter's, and little short of the height of the Pyramids. Another way of bringing home to our sensations an idea of the enormous size of these trees is that used by Messrs Sang. They calculate the quantity of wood in a tree, and its value at a penny per foot of inch deal. The result is L.6250 for a big one. What a nice little provision an acre of *Wellingtonia* would make for a younger son or daughter of the proprietor of an entailed estate!

Mr Lapham, the proprietor of the Mammoth Tree Grove, gives an interesting account, in the Kew Miscellany, of the dimensions of these trees. He tells us that most of the specimens now standing attain the average height of 300 feet; but one of them, known as the "Mother of the Forest," and stripped of its bark to the height of 116 feet for the purpose of being publicly exhibited, actually measures 327 feet in height and 90 feet in circumference. Enormous as these dimensions may seem, they are put in the shade by remembering what those of another tree must have been when in full vigour. This "Father of the Forest," as the specimen has been appropriately termed, has long since bowed his head in the dust, and now lies at length carelessly diffused. He still measures 112 feet in circumference at the base, and can be traced 300 feet where the trunk was broken by falling against another tree; it here measures 18 feet in diameter, and according to the average taper of the other trees, this giant must have been about 450 feet high, and was no doubt one of the loftiest vegetable forms of the present creation. A hollow chamber or burnt cavity extends through the trunk for 200 feet, large enough for a person to ride through. I may run shortly over the dimensions given by Mr Lapham of some of the other trees. "The Miner's Cabin" (for they have almost all received names) measures 80 feet in circumference, and is 300 feet in height. The "Three Graces," growing on one root, are 92 feet in united circumference, and 290 feet in height. The "Old Bachelor," which we are told is a forlorn-looking individual having many rents in the bark, and withal the most shabby-looking tree in the forest, is about 60 feet in circumference and 300 feet high. "Husband and Wife," leaning affectionately to one another, 60 feet in circumference and 250 feet in height. "Hercules," 97 feet in circumference and 325 feet high. "Addie and Mary" are each 65 feet in circumference, and 300 feet high. "Uncle Tom's Cabin," 75 feet in circumference and 300 feet high. They seem all to rise also like solid pillars, without a branch for nearly two-thirds of their height, often with furrowed bark, so as to look like fluted columns. The trees in Mariposa Grove are perhaps more various in point of age, but many of them do not fall much behind those of the Calaveros Grove in dimensions. One of those which I have figured from the photograph was 94 feet in circumference, and the butt, with the man leaning on it, shown in the woodcut placed at the beginning of this article, must have been still more. The smallest tree that could be found was 24 feet in circumference, and that of the next tree about 42 feet, and I shall tell the reader how I know.

My brother, last autumn, desired to obtain some seed of the *Wellingtonia* to send home. Now, this is not an easy thing. In the first place, the trees are greatly too high to allow of getting up them by any contrivance. I suggested flying a kite over them, and by that means getting a rope up the one side and down the other. Let any one fancy such an experiment being made over the Gas Company's chimney, and let him also fancy that after the rope was across, that he was the person to go up. I rather imagine he will not think it necessary for me to prove the inapplicability of my plan, unless, indeed, on the principle of hiring and sending out Steeple Jack (and he, poor man, I believe is dead, and has left no successors in his business). To cut down a tree was not impossible. It had been done already by speculators more than once to get a section for exhibition. I remember that in 1854 (shortly after the discovery of the tree), my brother was himself applied to to get a slice of it (not less than 30 feet in diameter) for exhibition in the Crystal Palace, and between L.300 and L.400 were placed at his disposal for this purpose. He found, however, that such a slice could not be got for the money—more particularly one of the *Wellingtonia*, because it was far inland, and the expense of getting it down to the coast would have been tremendous. The *Sequoia sempervirens*, however, grows in some places down to the water's edge, and this might have been more easily managed, and indeed was managed by some speculators, who exhibited at Philadelphia a section 12½ feet in diameter, taken 25 feet from the ground, which formed the basis of Dr Asa Gray's calculations as to the age of the *Wellingtonia*, and misled him regarding it, first from its being the *Sequoia sempervirens* instead of the *Wellingtonia* ; and, second, from the heart of the slice having been burnt out or removed, probably for the purpose of lightening the weight in carrying it about. Another section, or rather semidiameter, truly of *Wellingtonia*, was examined by Dr Torrey, 11½ feet in semidiameter —*i.e.*, 25 feet had the section been complete. Dr Seemann quotes an account of the taking of these sections, which is worth re-quoting, were it for nothing else but the impression which it leaves of the enormous size of the trees:—"The earliest account of the mammoth tree," says he, "which reached Europe were coupled with the sad intelligence that a piece of Vandalism had been perpetrated in Upper California unexpected in our enlightened days. One of the finest trees of the grove, we were informed, had been felled for the purpose of being publicly exhibited. This individual was 96 feet in circumference at the base, and solid timber. The work of destruction commenced by boring with augers, and sawing the spaces between—a labour engaging 25 men for five days. But when this was done, the tree was found to stand so nearly perpendicular that it would not fall; and it was only by applying a wedge and battering-ram, during a strong breeze, that the trunk was finally upset. In falling it convulsed the earth, and by its weight forced the soil from beneath it, so that it lies in a trench; and mud and stones were hurled near 100 feet high, where they left their mark on the neighbouring trees. A section of 2 feet long taken from the stump, also a portion of the bark, were both exhibited. The success with which the public exhibition of those specimens in San Francisco, New York, and Paris had been attended, induced, in 1854, another speculator to strip a second magnificent tree, the 'Mother of the Forest,' already mentioned, up to a height of 116 feet, of its bark, fortunately without affecting by this ruthless process the vitality of the tree. It required the labour of five men 90 days. During this time, a person had a fall of 100 feet from the scaffolding, and, curiously enough, escaped with a broken limb. The bark was removed in sections 8 feet in length, and each piece marked and numbered, so that it could be put up in precisely the same position that it occupied on the tree. It was then, after being carted 80 miles overland, shipped down the river to San Francisco, and thence on a clipper vessel round Cape Horn to New York, where, after being exhibited for a season, it was transmitted to London, and was for the first time on view (April 1856) in the Philharmonic Rooms, and afterwards at the Adelaide Gallery. But both of these localities were too low to admit of the whole sections of the stripped bark being put up, nor indeed was there any other available building in the British metropolis which could serve this purpose. Fortunately, the Crystal Palace at Sydenham possessed the necessary height; and ever since the autumn of 1856 the whole of the bark, to the height of 116 feet, has there been exhibited."

These quotations sufficiently show that, if one chose to be at the requisite labour and expense of cutting down a tree bearing cones, seeds could be thus obtained; but an obstacle to this mode of procuring them exists in the care that is now most properly taken to protect the trees and prevent their being exterminated. One would think that the difficulty of felling them would in itself have been a sufficient protection; but it was not thought so. Dr Seemann says—"It was at one time feared that not many years would elapse before the last vestige of the mammoth trees would be destroyed. It was the 'New York Herald' which first pleaded for their protection. In Europe the danger in which the trees were placed was viewed with equal apprehension, inducing a correspondent of the 'Gardener's Chronicle' to suggest that a petition of the scientific men might be sent to the American Government, praying for the protection of this eighth wonder of the world. Fortunately, the authorities were fully alive to their duty, by prohibiting the removal of any tree under any circumstances whatever, and thus, by throwing the sanctity of the law around the hallowed grove, preserved to North America an object quite equal in grandeur to the famed Falls of Niagara, the Mammoth Cave of Kentucky, or the Natural Bridge of Virginia."

The result of this is, that the only way of procuring seeds is to shoot down the cones with rifle bullets, or, so to speak, to saw off small branches with them; and my brother succeeded in getting Mr Patrick Black, a young Irish gentleman admirably fitted for such work, to undertake the task of procuring some seeds for him. A first-rate shot, a keen sportsman, full of energy, whom nothing delighted more than the exhilarating life of a hunter camping out for weeks in the open air, Mr Black was quite the right man in the right place. Well supplied with ammunition, he took his departure for the Mariposa Grove, which is a long way in the outer world—not that it is without its own inhabitants, its own hotel (kept by an old hunter), nay, even its own authorities, as Mr Black had like to find to his cost. He took up his quarters with the old hunter, who may rather be said to have kept *open house* than a hotel, as the sky was the only roof he had—a roof, apparently, not yet being considered essential to the comforts of a hotel in these parts, although one might have thought that it would, seeing that the forest is 6000 feet above the level of the sea, and there was frost every night while Mr Black was there.

He visited the grove daily, shooting down a cone or two to see that they were ripe before beginning to make his collection. He soon found, however, that it would take a battery of ammunition and an army of sharpshooters to make even a moderate collection of seeds. The seed is exceedingly small and thin, a mere scale, and the cone is also small (not much larger than the cone of an ordinary Scotch fir, and containing still fewer seeds), so that the product of a whole week's shooting might be held in one's waistcoat-pocket. Mr Black soon tired of this, and seeing one or two trees of less size than the others, and being apparently a man of a logical turn of mind, came to the conclusion, first, that it would be easier to fill his wallet by cutting down a tree than shooting down the cones; second, that it could be done; and, lastly, that as it could be done, it should be done; and being apparently also a man of a practical as well as of a logical turn of mind, he, boldly putting behind him the fear of the anathemas of the "New York Courier" and of the "Gardeners' Chronicle," as well as the nearer terror of the local authorities, at once, with the assistance of his host and two Frenchmen (that the three most civilised nations in the world might all be represented in the perpetration of the sacrilegious deed), proceeded to put his intent into execution. They first selected the smallest tree which they could find in the grove; it was 24 feet in circumference, and took Black and the hunter three days' hard work to level with the ground, one cutting on each side of the tree. Increase of appetite growing by what it fed on, another and another shared the same fate, until they had actually cut down four of these magnificent trees, the last and largest being 42 feet in circumference, which took a week to cut, and fell before the two Frenchmen; not, however, before the echoes of their axes reached the ears of Judge Lynch, who soon stopped the fun, and in simple but unmistakeable language gave him to understand that it would be "dangerous" to try it again. In plain English, the authorities interfered; and although they did not lynch Pat (which would not have set the trees up again), they told him that they would, if he cut any more. The time occupied in cutting down these trees would seem to indicate that that required to get the section of the tree at the Mammoth Tree Grove was either exaggerated, or unnecessarily long. Being twice the diameter, it might require four times the work; but twenty-five men for five days gives more than eight times the work. It also shows—what we see from the specimens of the wood itself—that the wood is extremely soft, very light, and easily worked, and not unlike the cedar-wood used for pencils; when freshly cut it is white, but speedily acquires the cedar hue. It is so *frush* (I am obliged to have recourse to a Scotch word to express my meaning, the English word brittle, which is nearest to it, scarcely conveying the full sense)—it is so frush, that one of the trees in falling snapped in three places before it reached the ground, carrying away whole forests of silver firs and pine before it; and we see from the figures of the trees which we already possess, as well as from the photograph of the group now appended, that a great proportion of them have been broken off near the top, so that if they had continued growing in the same proportion, they must have been nearly a third higher. But if the wood is frush, the bark is not. Our friends found it a great deal worse to cut through than the wood. It is tough and stringy like coir or the husk of a coco-nut, and is from a foot to a foot and a half in thickness. We have here one of those beautiful adaptations of structure to purpose which delight the mind to trace. It is obvious, that if the *Wellingtonia*, being so fragile, were coated with bark of only a common thickness and ordinary consistence, it could never live to be a tree; it would be snapped across by the first wind that blew, so soon as it reached a sufficient height to give the wind a hold upon its branches; but with a coating of bark so thick, so tough, so stringy, so spongy, and so elastic, it is kept in its place, and protected from its own fragility. It is the same principle which is adopted by ourselves in packing and supporting any thing that is fragile; and, as has been pointed out to me by my intelligent friend Mr Bryson, this support is given in the way which modern science has ascertained to furnish the greatest amount of strength with the least waste of substance. The bark is constructed on a different plan from that of most other trees,—it is on the plan of the corrugated roof, running longitudinally round the tree; the corrugated layers are composed of harder texture, and the interstices are packed with an elastic spongy substance.

Another adaptation of structure exhibited in this tree is the great gnarled expansion of its trunk at the base, which may be seen in the plate and vignette, thus supporting it against the wind by what may be styled a circle of buttresses.

I leave the reader to imagine the mingled feelings of dismay, chagrin, and satisfaction with which my brother greeted his triumphant emissary on his return (the *mens conscia recti* beaming on his face); and he now knows how I come to be able to give so accurately the dimensions of the *smallest* trees in the Mariposa Grove; as the lawyers say, "*causa scientiæ patet.*" The quantity of seed obtained, however, was by no means correspondent to the sacrifice made to obtain it. The cones on the trees would appear to have been comparatively few; and, as I believe is the case with other cypresses, the amount of light seed vastly preponderates. The whole quantity, good and bad, only amounted to between six and eight pounds; but as there are 50,000 seeds in a pound, the expedition has probably done more good than harm after all.

Another circumstance to be noted is, that the cone itself (that is, the woody part of the cone which envelopes the seeds) seems to be largely charged with a dark garnet or crimson-coloured gum. My brother, in sending me home the seeds, sent them carelessly cleaned, and a good proportion of what appeared to be seed was fragments of the cone itself; but in addition to that, there was no less than one-third part of the whole weight composed of this garnet-coloured substance, which had exuded from and had been rubbed off these fragments. My friend Dr Cleland has been kind enough to test it for me, and he informs me that it is entirely soluble in water; gives, with protosulphate of iron, a blue-black precipitate; with sesquimuriate of iron, a gray precipitate; and gives a precipitate with gelatine. It is thus a form of tannine, and may be called a sort of kino.

The portion of wood sent home by my brother gives me the opportunity of testing the calculations which have been made as to the age and rate of growth of the tree. It appears to have been taken from the exterior part of the tree, and contains 26 annular rings in an inch, in this respect nearly corresponding with the number recorded by Dr Torrey, as found by him in the outer part of the section he examined, where he found 20 annular rings in an inch. In his section the rings at the heart were found to be nearly twice as broad as they afterwards became. The first rings he found to be 6 in the inch, the last 20 in the inch; but immediately before the last 20, the rate was only 9 in the inch. The result to which Dr Torrey came was, that the tree was about 1200 years old, instead of 3000, as was at first improperly assumed, from reckoning only the outward rings, and taking it for granted that all the rings were of the same breadth. The tree, however, is obviously a fast-growing species, and has been shown by Mr Reed of Peterborough to make its growth between the hours of 6 P.M. and 6 A.M., and more rapidly or more slowly according to the warmth of the night. It is perfectly hardy in Britain, and has already reached the height of 14 feet at Martyr Castle, near Cork, and not much short of this both in England and Scotland, and has borne ripe fruit at Thetford in England. We may therefore reasonably hope that we shall ere long be independent of the sacred giants of the West for a sufficient supply of good seed. In the meantime we have the satisfaction of knowing that we can make plants by cuttings with the greatest facility; and what is most important in the great majority of cases, they grow erect and readily form leaders. Indeed, to any but a nurseryman's eye, it would often be difficult to distinguish between a seedling and a plant from a cutting of the same size. Should any of my readers like to be knowing on the subject, I would recommend them to compare the spread of the lower branches in the one with that in the other; it is not the cutting which usually has them broadest; but even this is a fallible test, depending greatly upon the kind of slip out of which the young plant has been made. This willow-like readiness to grow by cuttings is well seen, not only in the familiar fact above mentioned, but in various of the incidents which are to be observed in the Mammoth Tree Grove. Turning to one tree, the "Mother of the Forest," already mentioned as stripped of its bark to the height of 116 feet, we see it still flourishing, as we are assured by Dr Seemann. But I am inclined to think that the *Wellingtonia*, notwithstanding all its greatness, has no special exemption from the evil effects of girdling, and that by and by she will suffer from that fatal cause. But beside her lies her murdered lord, the "Father of the Forest," who we are told put forth several young shoots after he had been felled for some time; and there are few of her descendants standing around her in which great cavities (one as large as 17 feet across and 40 feet high) have not been burnt (either in consequence of fire raging through the forest, or kindled by Indians), and yet the trees do not seem to have suffered.

Dr George Lawson, in a paper which he read before the Edinburgh Botanical Society in March 1854, on the anatomical structure of coniferæ and other gymnogens, noticed the microscopical structure of the *Wellingtonia gigantea.* He stated that he found it to present a double row of opposite discs, which, as well as their central dot, were elliptical. I have been enabled to verify Dr Lawson's observation through the kindness of Mr Bryson, who has made sections of the wood now received, and carefully examined them. He says, "I find the structure on the transverse section resembles very much the *Taxodium distichum* (deciduous cypress), although the reticulations are larger. The radial longitudinal section exhibits the coniferous discs perhaps better than any other wood I have examined. The discs lie side by side, and do not alternate as in the Araucarias; they are more oblate than in the true pines, and seldom occur in double rows; on an average 28 rows of discs occur between the walls of each cell. In *Taxodium distichum*, 20 rows obtain on an average." In *Sequoia sempervirens* the discs are round and not oblate.

SEQUOIA SEMPERVIRENS, Lambert.

The incidental remarks which I have made upon this tree in contrasting it with the *Wellingtonia gigantea* have anticipated most of what I had to say regarding it. It was first discovered by Menzies in 1796, and is found as far south as the Santa Cruz mountains, near Monterey, as well as considerably to the north of San Francisco. The character of the tree is well described by Douglas, in the passage which has been supposed to indicate that he saw the *Wellingtonia*. "The great beauty of Californian vegetation," he says, "is a species of *Taxodium*, which gives the mountains a most peculiar—I was almost going to say awful—appearance, something which plainly tells us we are not in Europe." This of course refers to a tree so common as to give a tone to the general scenery of the country, which we know *Wellingtonia* is not, while the *Sequoia sempervirens* is. As already mentioned, it is nearly as large and tall as the *Wellingtonia*. One tree, called by the settlers the "Giant of the Forest," is 270 feet high, and 55 in circumference at 6 feet from the ground. It is known to the settlers as the red-wood—a name which I find appended to the *Wellingtonia* by Dr Seemann, but this is obviously an error, or a mere extension of the name, for the wood of the *Wellingtonia* has probably never been used for economic purposes by the settlers, while the other is largely used. It is of a beautiful red colour, fine and close grained; but light and brittle, like the *Wellingtonia*. It is good for purposes where it is exposed to water, as some of our own soft woods are, and is said never to be attacked by insects. Its bark is thick, and even in the young tree is soft and spongy.

There is another point regarding it worthy of being noticed—viz., its great probable age; I do not mean the age of the individual trees, although that is by no means contemptible—a slab of the wood deposited by Dr Fisher in the St Petersburg Museum, measuring 15 feet in diameter, and showing 1008 annual rings—but I refer to the age of the species. M. Lesquereux, who has paid much attention to the subject, conceives that he has identified this tree among the fossil remains of the tertiary deposits of Vancouver's Island. The idea that it gave a character to the landscape in these bygone ages, long before the eye of man was present to take cognisance of them, cannot fail to excite a feeling of interest even in the least poetic.

PINUS INSIGNIS, Dougl., *and* PINUS RADIATA, Don.

A suspicion has been gaining ground among botanists that —like many other pines which in their extreme forms look very distinct, *Pinus insignis* and *Pinus radiata* may be found to be synonymous. Hartweg seems to have had this in his mind when he named *Pinus radiata, Pinus insignis macrocarpa.* Mr Gordon, however, who in his recent valuable work on Coniferæ (the Pinetum) shows little inclination to spare doubtful species, decides in favour of their both being good; and as he does in this instance what he has usually abstained from doing, viz., gives a reason for his judgment, we are enabled to form an opinion for ourselves as to the correctness of the result to which he has come.

He says of *P. radiata:* "This beautiful pine resembles *P. insignis* in some respects, but differs very much in foliage and cones; the leaves of *P. insignis* are much longer and stouter than those of *P. radiata,* while the cones of *P. radiata* are nearly three times the size of those of *insignis* and with the scales much more elevated." Now, unless the difference in the length and stoutness of the leaves be very marked (which is not the case here), I think we can hardly attach much importance to this as a character. So much depends upon the health of the plant, the part of the tree whence the leaves are taken, &c., that the most different degrees of length and stoutness of leaf may be observed in the same tree. As to the cones, they certainly differ widely, but I have received and presented to the Edinburgh Royal Botanic Garden Museum a branch encircled with a cluster of five cones, three of which are of *P. radiata* and two of *P. insignis,* each typical of the extreme form characteristic of the two so-called species—the only exception being that they are here both of the same size—about 4 to 4½ inches in length, instead of the *P. insignis* being 3½ and the *P. radiata* 6 inches in length, which are said to be their usual dimensions.

Both varieties are found in the same district (the Monterey district, to the south of San Francisco) and their general appearance and the colour of their foliage is the same. This we would perhaps not find out from Mr Gordon's description, because he calls the one deep grass-green, and the other deep green, a discrepancy in describing the same thing which must have escaped him, for he afterwards refers to Hartweg's description of the beauty of the "deep grass-green" of the foliage of the one which he had just described as deep green. It is very essential for a describer to take care that he always uses the same term or phrase to designate the same thing. I could point to many who actually go out of their way to find another word to express the same quality, intending thereby to escape the harshness of constant repetition. But people do not expect euphony in scientific descriptions; what they want is clearness, and how can that be obtained when different terms are used to express the same thing?

The specimen mentioned already as having been presented to the Botanic Garden will, I imagine, satisfy every one that these two names are only synonymes of the same tree, indicative of the different states in which it is found, owing probably to difference of soil, climate, position, &c.

Regarding, as I have done, *P. insignis* and *P. radiata* as one species, I may state that this tree has now been cultivated for a considerable time in Britain, and is greatly admired for its lovely green hue and soft foliage. Gordon says it is perfectly hardy. In the south of England it is undoubtedly so. It is a great favourite in Devonshire, where trees may be frequently seen between 30 and 40 feet high; and I believe there is a specimen in the garden of Messrs Lucombe, Pierce, and Co., nurserymen, Exeter, which is nearly 50 feet high. It has not yet, however, been satisfactorily established to be hardy in Scotland. By care and protection it has been reared to a considerable height, but some sudden spring frost seems always sooner or later to cut it off at a time when all danger has been thought past, and all that remains of the care of years is a pile of rust-coloured leaves in place of the tender green which yesterday delighted the eye.

The finest plant with which I am acquainted near Edinburgh, is one in Mr Samuel Hay's lawn at Trinity Lodge. It is a beautifully shaped conical tree, nearly 13 feet in height. The next in height, perhaps, is one at Mr George Logan's of Duddingston, which is about 11 feet.

Mr Humphrey Graham, of Belstane, in the Pentland Hills, from whom I have received much valuable information regarding pines (although of too practical a nature to be introduced into this Journal), under the disadvantage of an almost subalpine climate 800 feet above the level of the sea, promised to be more successful in rearing them than any other person in the middle or northern district of Scotland with whom I am acquainted, but even he had all his plants but two swept off by the frost in 1856. He is not discouraged, however, and he reports to me that he is still satisfied it will succeed in Scotland, if tolerable care be taken.

The timber is not good. I remember my brother telling me when he was last in this country that it was useless. It would appear, however, that a use has now been found for it. In a recent letter he writes, "the street planking here (San Francisco) used to be done with Oregon lumber, but now it is being superseded by the Monterey lumber (most likely *P. insignis*) for the reason that it is very resinous, and stands the wear and tear of such a purpose better."

PINUS JEFFREYI, Oreg. Com. (Plates VIII. and IX.)

This pine was discovered by Mr Jeffrey, who was sent out in 1850 to collect seeds in North West America by an association of gentlemen which originated in this city, and was principally composed of Scotchmen, although it also numbered in its body many noble and eminent subscribers from the sister kingdom, chief of whom I should mention, His Royal Highness the Prince Consort. That association still lives in its embers, and I trust that an effort now making to revive it may be successful, and that it may yet make as many discoveries in Japan as it did through Jeffrey in Oregon and California. Some subscribers to the association remembering only that the third and last year of Jeffrey's engagement terminated unsuccessfully, and that they had just reason to be dissatisfied with his conduct during that year, sometimes speak of his expedition as a failure. But it is unjust so to term it; and if they would only remember the quantities of novelties which were discovered and introduced through his means, they would rather treat it as a great success, which only assumes the aspect of a partial failure from the knowledge that, great as it was, it ought to have been, and might have been, greater still. No one could have worked more conscientiously and more perseveringly than Jeffrey did during the first two years of his employment, and bearing in mind the fact that Menzies and Douglas went to a virgin country, his collections do him no discredit, even as compared with theirs. He discovered several new pines, six of which were described by Professor Balfour, along with figures by Dr Greville, in one of the Reports of the Oregon Committee, and two or three more still remain undescribed. The Report of the Oregon Committee having been only issued to its shareholders, cannot, perhaps, be strictly said to be published, at least I understand that some scientific purists so maintain, although I am not sure that they are right, since I see little difference between a printed report to an association sent to its subscribers, and a printed book (published by subscription) sent to its subscribers. But be that as it may, Mr Gordon has published the description of this pine in his work, and its name and identity are thereby secured. He has not, however, given the figure of the cone, which is one of the most perfectly beautiful I have ever seen.

As I have received a sketch of the tree itself, taken by Mr Peebles, which I have caused to be lithographed for this paper (Plate VIII.), I have thought it desirable at the same time to reproduce the figure of the cone (Plate IX.)

Jeffrey found the tree in Shasta Valley, North California, lat. 41·30°. It has also been found in Scots Valley; and Mr Black, whom I shall have presently to mention, found it near Mariposa. It is a fine tree, 150 feet in height, and 4 feet in diameter. It has not yet been found near enough any of the cities to allow of the economic value of its wood being ascertained.

PINUS MURRAYANA, Oreg. Com.

This is another of the species discovered by Jeffrey, and described by Professor Balfour in the Report of the Oregon Committee. Mr Gordon, however, disallows it, placing it as a synonyme of *P. muricata,* but without stating the grounds on which he has come to that opinion. It appears to me very distinct; and although I have, as in the case of *P. radiata* and *P. insignis*, the advantage of additional and better material to form a judgment upon than probably was in Mr Gordon's hands, I can scarcely acquit him of hastiness in coming to the conclusion he has arrived at. He gives a correct account of the locality where the true *P. muricata* was found, viz., in the mountains of Monterey, mountains not higher than 3000 feet, and situated near the sea, and south of San Francisco; and also states correctly where Jeffrey had found, what the Oregon Committee called *P. Murrayana*, viz., on the Syskyon Mountains, far north of San Francisco, at an elevation of 7500 feet above the level of the sea; and the tree is described as being at both of these places about 40 feet high. Now, one of the facts with regard to the distribution of conifers in California, which must have struck any one who has studied the subject, and with which Mr Gordon cannot fail to be familiar, is that, taking San Francisco as a point, the pine vegetation to the north and south of it, making a certain allowance for transitional portions, is essentially distinct. In the latitude of San Francisco, we have the *P. Sabiniana,*—to the south of it, *P. Coulteri, P. insignis, P. muricata, P. bracteata*, &c. North of it their place is supplied by *Picea nobilis* and *grandis, Pinus monticola, P. tuberculata, P. Jeffreyi*, &c.; and the very circumstances of the one being found at an elevation of 7500 i / it feet, so far north as the Syskyon Mountains, and growing near the sea, at an elevation of 3000, so far south as Monterey, ought to have put Mr Gordon on his guard against confounding them.

Without going into minute detail as to the differences between the two, I shall only observe, that the cone of *P. muri-*

cata is 3 inches in length; while a pretty extensive series of *P. Murrayana* enables me to say, that its dimensions are from 1½ to 2 inches in length. Again, *P. Murrayana* has a very peculiar long spine, or rather prickle, from 1 to 2 lines in length, sticking outwards and backwards from the middle of each scale; while *P. muricata* has only "a slight ridge running across the scales near the top, terminated by a short, straight broad prickle in the centre." In the specimens of *P. Murrayana* which were received from Jeffrey, these spines were broken off, and the cone is so figured, and Mr Gordon is not responsible for the error thence arising; but they are well marked in specimens since sent, more than once, by my brother, and now in the Museum of the Botanic Garden, and in that of Messrs Lawson.

Mr Gordon says, that it is the Obispo or Bishop's Pine, and perfectly hardy. This is only half true. The *P. muricata* is the Bishop's Pine, and the *P. Murrayana* is perfectly hardy. That the *P. muricata* is hardy, is more doubtful.

There is another pine known to horticulturists as *P. M'Intoshiana*, which Mr Gordon considers synonymous with *P. contorta*, Don, but which I think is more likely to prove synonymous with *P. Murrayana*. In the young state they are undistinguishable; but I have not seen the cone of *P. M'Intoshiana*.

Mr Black, an English engineer who had occasion, in the performance of works entrusted to him in California, to make use of various of the country woods, informs my brother, that *P. Murrayana* is the best wood in the country for railway sleepers, sluice-heads, and purposes where a hard and durable wood is required; but being of a small growth, and more knotty than some of the others, is not so good for planks, and what is technically known by the term lumber. He also mentions as a peculiarity in it, that the rings are more concentrated at the outside than at the heart, which he says is just the reverse of the others,—only of some of them however,—for we shall find that this is also the case with *Wellingtonia gigantea*. He suggests that it may indicate a rapid growth when young, and slow afterwards, owing, perhaps, to the scantiness of the soil in the rocky regions where it grows.

On the Incorrectness of the Present Mode of Estimating the Mean Temperature in England. By JAMES STARK, M.D., F.R.S.E., &c., &c.*

As in other sciences, meteorologists are very apt to be led by a name; and provided a practice be recommended by some one who, from his position or otherwise, has acquired a name, his recommendation is adopted without examination, even to the damage of the science itself. Such is the case with regard to the mode of estimating the mean temperature at present followed in England. Mr Glaisher, who occupies the position of Meteorologist in Greenwich Observatory, and Secretary to the Meteorological Society of England, has recommended, and has for years followed, the practice of *estimating* the mean temperature by taking it as the mean of the united observations made by the self-registering and common thermometers—the exact mean of the observations made by each of these instruments being first *altered* by certain tables which he has constructed for the purpose of correcting them for what he terms *diurnal range.*

Let us look into this subject a little, and see the facts on which such corrections are made; and the principle involved in making the *altered* observations of one instrument made the basis for the correction of another and more trustworthy instrument, whose indications are at all times steady, and free from the liability to error to which the other is subject if not read at the exact hour and minute of time when the observation ought to have been made.

At Greenwich, from 1840 to 1845, a series of thermometric observations was made with the common dry-bulb thermometer alongside of the wet-bulb, the readings being taken every second hour of Göttingen mean time. It was inferred, but without any proof that such is the case, that the mean of these twelve readings in the twenty-four hours would give the true mean temperature of the day, although it was known that these readings would almost invariably miss the period when the greatest heat of the day occurred; so that from this circumstance, the mean temperature so deduced would *always be below the truth.* It was also inferred that the readings of the dry bulb would not be at all influenced by the neighbourhood of the wet bulb, the evaporation from which would also have the effect of causing the dry-bulb readings to be slightly *below* the truth. It was further inferred, that the rise and fall of temperature was uniform between each two-hourly period at which the readings were taken; so that, when tables were drawn up to suit Greenwich time, the corrections to be applied were calculated on the principle of uniform rise and fall, which we all know is far from being the case. It was also inferred, that a period of five years would suffice to deduce a satisfactory mean basis on which all the calculations should be founded,—a fallacy which strikes at the root of the whole superstructure.

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* Read before the Royal Society of Edinburgh, 16th January 1860.

Now, any one reflecting on these circumstances will at once see that serious errors must arise from constructing tables of correction on such imperfect data—*Firstly,* Because a five years' series of observations is far too short a period to give a satisfactory mean result for either daily, monthly, or annual mean temperature; and, *secondly,* Because the highest temperature of the day not being noted at all by these two-hourly readings, we not only obtain a mean result *below* the truth, but we have no data by which to calculate the period of the day when the highest temperature is attained; and hence the 3 o'clock readings are rendered perfectly useless for the purpose of estimating mean temperature, when *three* readings only are taken in the course of the day, of which that at 3 o'clock is always one.

First, then, it may be demonstrated that a series of observations for a period of five years only could never elicit a true mean, either for days, months, or years; and yet, as the basis of all tables of correction must be founded on a fixed mean, it is absolutely necessary that that basis be extended over a much longer period of years before any reliance could be placed on its deductions. Let us look for a moment at this point of the subject.

Scotland has only had the advantage of having its meteorological data collected and published since 1855. Let us see how the last four years stand with regard to the one element of mean temperature:—

SCOTLAND.

Mean temperature for	1855,	. .	44·1°
„ „	1856,	. .	45·7
„ „	1857,	. .	48·0
„ „	1858,	. .	46·6

It is seen at once from this table, that during this period of four years there has not been the most distant approach to uniformity of even mean annual temperature in Scotland; but, on the other hand, there is the striking fact that during that period the mean temperature of one of the years differed from that of another by nearly 4 degrees. Now it would be a most unsafe conclusion to deduce from this table that the mean temperature of Scotland was 46°·1. The period of time is evidently far too short to afford even an approximation to the truth.

If we look at the mean temperature of a few of the months during these several years, we shall find a still greater diversity, as is shown in the following table:—

SCOTLAND.

Years.	Feb.	March.	May.	June.	July.	Sept.
	°	°	°	°	°	°
1855, . .	27·0	36·0	44·0	55·0	60·0	52·0
1856, . .	39·6	39·4	46·7	53·3	56·4	50·9
1857, . .	39·3	39·2	49·8	57·4	58·0	56·1
1858, . .	35·8	39·5	49·5	58·9	56 0	54·5
1859, . .	39·6	43·0	51·9	56 9	59·5	52·3
Range, . .	12·6	7·0	7·9	5·6	4·0	4·1

By this table it is seen that during a period of five years the range of mean temperature during the months has been so varied in different years as quite to preclude the idea of a five years' average giving even the most distant approach to a true mean. Thus, in July and September, the range of mean temperature during five years was 4 degrees; in June, 5·6°;

n March, 7°; in May, 7°·9; while in February it was no less han 12°·6.

Secondly, It may next be shown that the two-hourly periods of observation could not give the true mean temperature, as they did not include the highest temperature of the day, which was from two to six degrees higher than any of the two-hourly readings, during the warmer periods of the year. In proof of this position is subjoined one of the Greenwich Summary Tables for the year 1844, one of the years on which Mr Glaisher's Tables of Correction are founded; and this table speaks a language which one would think no one can misinterpret.

This table shows the highest and the lowest degrees of temperature at Greenwich during every month of the year 1844, both as noted at the two-hourly readings of the dry-bulb thermometer, and also as indicated by the self-registering thermometers; and a comparison of the highest readings of the dry-bulb, with the highest readings of the self-registering thermometer, and likewise the comparison of the lowest readings of the dry-bulb with the lowest readings of the self-registering thermometer, fully bear out the position which has been stated. Thus, every meteorologist knows, that while the curve of temperature is very great during the day, the curve of temperature during the night is so much less that for several hours of the night the temperature varies but little. This table, then, exhibits also this fact; for while it shows that the two-hourly readings approached the lowest temperature during the night within a few tenths of a degree, on the other hand, these same two-hourly readings never, during any month, reached the highest temperature, the very mean of the twelve months showing that the highest readings of the dry-bulb thermometer at the two-hourly periods were 2°·4 *below* the actual highest temperature which occurred, as indicated by the self-registering thermometer.

Greenwich.	Dry-Bulb Thermometer.			Self-Registering Thermometer.		
1844.	Highest reading.	Lowest reading.	Range.	Highest reading.	Lowest reading.	Range.
January, . . .	52·8	18·6	34·2	53·7	18·8	34·9
February, . . .	48 3	21·2	27·1	50·4	20·0	30·4
March, ...	57·6	24·3	33 3	60·2	24·1	36·1
April, ...	74·6	34·0	40·6	74·9	33·4	41·5
May, ...	74·6	34·6	40 0	77·4	33·9	43·5
June, ...	83·3	43·6	39·7	87 6	43·4	44·2
July, ...	85·1	47·2	37·9	87·4	47·1	40·3
August, . . .	72·5	43·1	29·4	75·4	42·8	32·6
September, . .	73·1	35·2	37·9	78·0	34·8	43·2
October, . . .	65·9	31·5	34·4	67·4	30·8	36·6
November, . .	57·4	28·8	28·6	58·1	27·4	30·7
December, . .	46·0	21·6	24·4	49·3	21·1	28·2
Mean, . .	65·9	31·9	33·9	68·3	31·4	36·8

This table, then, at once proves, what was stated at the outset, that readings at two hourly periods must fail to give the true mean temperature, and must indicate a mean temperature *below* the truth. But what is true of the months is also true of the days, and this, of course, to a greater extent than the months themselves. Let us take an example from the Greenwich observations of the highest temperatures by each kind of instrument on consecutive days of the same month of 1844. On the 2d of September, the highest of the two-hourly readings of the dry-bulb thermometer was 73°·1, but the actual highest degree of temperature which occurred that day was 78°, as registered by the self-registering thermometer. On the 3d September, the highest two-hourly reading was 68°·7, but the actual highest degree of temperature that day was 71°·4. On the 4th September, the highest two-hourly reading was 72°·8, but the actual highest was 73°·7. On the 5th September, the highest two-hourly reading was 69°·1, but the actual highest was 71°·3. On the 6th September, the highest two-hourly reading was 72°·2, but the actual highest was 73°·8. On the 7th September, the highest two-hourly reading was 72°·5, but the actual highest degree of temperature was 74°. These, then, may serve as examples of the statement made, and at once account for the mean of the two-hourly readings being below the mean of the self-registering thermometers, *and of course below the truth.*

Had Mr Glaisher, however, confined his corrections to the dry-bulb readings alone, no great harm might have resulted, provided these corrections were applied to their legitimate use —viz., to enable a person from one reading of his thermometer daily to deduce the *probable mean temperature* of his locality for each month. But Mr Glaisher has gone far beyond this; for he not only alters by these tables every observation made with the dry-bulb thermometer, so as to let the mean of no series of observations be published *as observed*, but, from finding that the mean of his two-hourly readings was always *below* the mean of the observations made with the self-registering thermometers, he alters their means also, by deducting from the mean of each month a quantity which is intended to reduce their mean to the mean value of his two-hourly readings. He thus commits two great errors—*first*, in assuming that the two hourly readings of the dry bulb, taken in the faulty circumstances above noticed, give the only true mean temperature; and, *secondly*, that the mean of the self-regising observations must be erroneous. He hence asserts that the mean temperatures procured by taking the strict mean of the maximum and minimum readings of the self-registering thermometers are too high. I shall however prove, I trust to your perfect satisfaction, that the strict mean of the maximum and minimum self-registering thermometers (provided these instruments be of proper construction) is far nearer the true mean *when unaltered*, than when altered by Mr Glaisher's tables.

Now, to prove this point, I am prevented from referring to the Greenwich observations, *first*, because the original observations, from which Mr Glaisher drew his conclusions, were made with that most untrustworthy instrument, Six's registering thermometer, so that I cannot refer to the observations made with it; and, *secondly*, because when, at Greenwich, instruments of proper construction were procured, Mr Glaisher has published no results which could permit any one to test the accuracy or applicability of his tables. He has not only abstained from publishing the mean results of the dry-bulb observations, but also those of the wet-bulb; and contents himself with publishing certain deductions alone, and an *estimated* mean temperature, a compound result of altered *dry bulb* and *altered self-registering thermometric* means—and this result deduced on a principle which every one who examines it must condemn. This practice, in a public observatory, whose observations are made and published at the public expense, is strongly to be condemned. Whatever *corrected* (?) mean results, or deductions from these, are published, the strict means of the whole of each series of original observations, freed merely from instrumental errors, ought to be given in full at the same time, in order that others, who consider the tables used for their correction to be erroneous, or who, though they admit the principle of the correction, may consider a five years' period of time far too short to give trustworthy means, or who deny that corrections proper for Six's instrument are at all needed for those constructed on Rutherford's, or Negretti's, or Phillip's principle, or who may wish to verify the accuracy of the corrections applied, may have it in their power to examine the subjects for themselves. Besides, the calculator may have blundered his calculations; he may have added a correction when he ought to have deducted it, and the estimated results, as published, may differ widely from the truth.

But it is not only the Greenwich observations which are altered in this manner. Mr Glaisher, from his position as Secretary to the Meteorological Society of England, alters in the same manner the results from all the fifty-five Meteorological Stations in England; and as he similarly withholds the strict means of the different series of observations made with the dry and wet bulb thermometers, he prevents all inquiry as to the correctness of the alterations which he makes.

In order, then, to prove that the mean temperature as deduced by taking the strict mean of the maximum and minimum readings of the self-registering thermometers, *is far nearer the true mean* than when altered by Glaisher's Tables, reference will be made to the Scottish series of observations, which are made with trustworthy instruments; and which, from their very number, ensure an accurate mean result, much more truly than if they were the limited observations made at one station. But in Scotland we have, in addition, a series of observations carried on at Makerstoun, by our late venerable and honoured President, Sir Thomas M. Brisbane, with a care and accuracy which nothing can surpass. Reference will therefore also be made to the Makerstoun observations, for the years 1857 and 1858, to illustrate and prove the same point.

The following table gives the mean readings of the maximum and minimum self-registering thermometers, and alongside of these, the mean of the morning and evening 9 o'clock readings of the dry-bulb thermometer at Makerstoun, for the several months of the years 1857 and 1858:—

MAKERSTOUN.	1857.		1858.	
Months.	Mean of Self-regist. Thermoms.	Mean of Dry-bulb Thermom.	Mean of Self-regist. Thermoms.	Mean of Dry-bulb Thermom.
January, ...	36°·5	35°·3	38°·8	38°·5
February, . . .	39·1	38·2	35·1	34·8
March, ...	39·4	39·1	40·0	39·4
April, ...	42·7	42·3	44·1	44·0
May, ...	49·3	48·6	49·2	49·2
June, ...	57·0	57·1	59·4	59·6
July, ...	56·2	58·2	56·5	55·7
August, ...	60·5	60·2	57·5	57·4
September, . . .	54·9	54·9	55·2	53·5
October, ...	50·6	49·4	46·0	45·2
November, . . .	43·0	43·0	39·7	38·0
December, . . .	44·9	44·5	39·0	38·2
Mean, . . .	47·8	47·5	46·7	46·0

By this table it will be seen, that at Makerstoun, in the south of Scotland, the mean temperature, as given by the 9 o'clock morning and evening readings of the dry-bulb thermometer, is so very close on that of the self-registering mean, that the difference might almost result from the mode of taking the readings. The tenths of a degree are all estimated by the eye, and according to the level at which the instrument is presented to the eye, there may easily be differences of several tenths of a degree. The slight differences, however, between the means of the two kinds of instruments may have been caused by the Sunday readings having been included in the readings of the self-registering thermometers, whereas no Sunday observations are taken with the dry-bulb thermometer.

Let us, however, suppose for a moment, that the Greenwich Table of Corrections,* *for the dry-bulb* reading at 9 o'clock, applied to Makerstoun. On the mean of the year, they would add six-tenths of a degree to the mean of the dry-bulb readings, and thus bring the mean of the dry-bulb for the year 1857 to 48°·1, and that of 1858 to 46°·6. To procure a mean result, however, for these years we may, for the sake of illustration, unite the means, and divide by two, when we get, as the mean result of the two years, the mean temperature of the dry-bulb corrected by the Greenwich Tables, as 47°·3; while the mean of the self-registering thermometers as observed, and without any correction whatever, is within one-tenth of a degree of the very same—viz., 47°·2. This fact of itself clearly proves, that the mean temperature which is the strict mean of the self-registering thermometers (when these are of proper construction) is the true mean, and requires no correction whatever.

This fact is still better and more satisfactorily demonstrated by the Makerstoun observations for 1844, the last of the Makerstoun Meteorological Reports published by the Royal Society, which contains SUMMARY Tables of the *hourly* readings of the common thermometer. In the annexed table, the Sunday readings of the self-registering thermometer are excluded, in order to render the results of dry-bulb and self-registering thermometers thoroughly comparable,—no readings of the dry-bulb being taken on Sunday.

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* Two tables for diurnal range have been drawn up in Scotland—viz., for Leith and for Culloden. The data on which these are founded are too imperfect to render them trustworthy. They however demonstrate, that the horary (miscalled diurnal) range of temperature is very much less in Scotland than at Greenwich.

MAKERSTOUN, 1844.

Months.	Mean of Self-regist. Therm.	Mean of Dry-bulb hourly Readings.
January, ...	33°·63	36°·92
February, ...	32·47	32·22
March, ...	38·60	38·23
April, ...	47.08	46·60
May, ...	48·69	48·46
June, ...	55·26	54·20
July, ...	56·07	55·56
August, ...	55·87	54·32
September, ...	53·02	52·46
October, ...	46·33	45·71
November, ...	41·88	42·66
December, ...	31·73	31·63
Mean, ...	45·05	44·93

By this table it is seen that the mean annual temperature, as taken by the *hourly readings* of the dry-bulb thermometer, was 44°·93, while by the self-registering thermometer it was 45° Fahr., a correspondence so close that it is scarcely possible to get a nearer approximation. Here, then, we find that *hourly readings* of the dry-bulb thermometer give an annual mean temperature within one-tenth of a degree of the strict mean of self-registering thermometric observations; and as we all know the care and accuracy with which the Makerstoun observations are made, even if this fact stood alone, it would prove the point contended for—viz., that, in so far as yet appears, the strict mean of the self-registering thermometers, *when these are of proper construction*, give the true mean temperature. It may be remembered, that all the tables of correction which different meteorologists in this country and on the continent have published, for the purpose of correcting the mean values of the self-registering thermometers, were drawn up from observations made with that untrustworthy instrument, Six's self-registering thermometer—an instrument so notoriously untrustworthy, that it would not now be received into any observatory in this country.

These results, then, appear to demonstrate, in the most satisfactory manner, that *in Scotland the self-registering thermometers*, if of proper construction, *give the true mean temperature and require no correction whatever.* This conclusion is rendered still more certain by taking the mean of all the observations made in Scotland with the two sets of instruments during the years 1857 and 1858.

ALL SCOTLAND.	1857.		1858.	
Months.	Mean of Self-regist. Thermoms.	Mean of Dry-bulb Thermom.	Mean of Self-regist. Thermoms.	Mean of Dry-bulb Thermom.
January, ...	35°·7	35°·6	39°·3	39°·3
February, . . .	39·3	39·2	35·8	35·1
March, ...	39·2	38·9	39·5	39·0
April, ...	42·7	42·8	43·8	43·6
May, ...	49·8	49·7	49·5	49·7
June, ...	57·4	57·6	58·9	59·0
July, ...	58·0	58·1	56·0	56·1
August, ...	60·0	59·7	57·9	58·1
September, . . .	56·1	56·0	54·5	54·4
October, ...	49·6	49·4	44·9	44·3
November, . . .	43·7	43·5	39·4	38·8
December, . . .	44·9	45·0	39·9	39·9
Mean of Year, .	48·0	47·9	46·6	46·5

This table, then, affords an additional demonstration of the fact that, in Scotland at all events, *the mean of the self-registering thermometers gives the true mean temperature*, and requires no correction whatever for any supposed monthly variation. It also shows, what the Leith and Makerstoun observations had previously demonstrated, that the 9 o'clock morning and evening readings of the common thermometer give a very close approximation to that mean; so close, indeed, that for all ordinary purposes their mean results might be used wherever the means of the self-registering thermometers were unattainable.

But, as before remarked, Mr Glaisher would allow no result to be published *as observed*, but would apply to all results some fancied correction. Having, therefore, made his correction on the dry-bulb thermometer, to bring it to what he terms its true mean, he proceeds to do the same to the mean result of the self-registering thermometer. As his faulty observations at Greenwich made it appear that the Six's self-registering thermometer always gave too high a mean temperature, in order to reduce that mean to strict conformity with his supposed true mean, as given by his two-hourly readings of the dry-bulb thermometer, he deducted from the self-registering mean a quantity equal to the difference between the mean readings of these two instruments. On the mean of the year that deduction amounts to 1°·07; so let us for a moment see what effect Mr Glaisher's *corrections* (?) would have on our Scottish observations—whether they would cause them to agree more closely, *which if they were really corrections they would do*, or whether they render both mean temperatures unquestionably incorrect and quite diverse from one another.

The following table, then, exhibits the mean results at Makerstoun and for all Scotland for the years previously quoted, with the corrections to each set of instruments required by Glaisher's Tables, and the asserted UNIFORMITY sought for by the application of these tables:—

	MAKERSTOUN.						SCOTLAND.			
	1844.		1857.		1858.		1857.		1858.	
	Mean of Self-registering.	Mean of hourly reading, Dry-bulb.	Mean of Self-registering.	Mean of 9 Morn. & Even., Dry-bulb.	Mean of Self-registering.	Mean of 9 Morn. & Even., Dry-bulb.	Mean of Self-registering.	Mean of 9 Morn. & Even., Dry-bulb.	Mean of Self-registering.	Mean of 9 Morn. & Even., Dry-bulb.
Mean temperature as observed, ...	45·05	44·93	47 80	47·5	46·70	46·0	48·00	47·9	46·60	4·65
Corrections for each by Glaisher's Tables, ...	—1·07	0·00	—1·07	+0·6	—1·07	+0·6	—1·07	+0·6	—1·07	+0·6
Results,...........	43·98	44·93	46·73	48·1	45·63	46·6	46·93	48·5	45·53	47·1

By this table it is seen that, instead of Glaisher's corrections causing the observations made by the two kinds of instruments to agree, it causes them to differ most widely from one another. Yet the very object of applying these corrections at all was for the express purpose of making them correspond.

This table, therefore, it is hoped, will satisfy even the most prejudiced, that to alter by Glaisher's Tables the means as observed, or, to use scientific language which mystifies while it prevents inquiry, "to apply a correction for diurnal range," is only propagating error. This table, above all, shows the error of altering the means of the self-registering thermometers, which were all correct before being altered by Glaisher's Tables, but are just as manifestly rendered incorrect when changed by these tables.

Now, seeing this is undoubtedly the case in Scotland, and is proved to be so even with one of its most southern stations, Makerstoun, it is nearly certain that such will also be found to be the case with nearly all, if not all, the stations in England, including Greenwich itself. We are precluded, however, from demonstrating that such is absolutely the case, by the circumstance that since this mode of altering the mean results has been adopted, Mr Glaisher, both in the Greenwich Observations, and also in the Meteorological Reports and Tables for all England, has ceased publishing the observed means of all the different series of readings from the dry and wet bulb thermometers, so that he puts it out of the power of any one to test the applicability of his correctness.

It ought to be laid down as a principle, that, if the mean temperature is to be estimated at all, it ought to be deduced from the series of observations made with one form of instrument alone—and the one least liable to error is the self-registering thermometer, constructed according to Rutherford's, Phillip's, or Negretti's principle; observations made with Six's thermometer being utterly worthless. There is thus avoided all source of error from not being certain of the amount of diurnal range which exists at each station, or from neglect to read the instrument at the exact minute of time when it ought to be read—sources of error which always exist when the dry-bulb readings are made use of. This mode of estimating the mean temperature also renders easy and certain the comparison of temperatures at different stations, or different parts of the world; it also avoids all those errors which arise from blunders on the part of the calculator, who, if he makes use of tables, is apt to add a number when he ought to deduct it, or deduct it when he ought to add it; and who, if he used Glaisher's

Tables, as originally published in the "Philosophical Transactions," would be sure unwittingly to commit mistakes, as these tables are themselves not free from serious blunders.

What, however, I most especially desire to effect by directing attention to this subject is, that in all the Meteorological Reports which are published, the strict mean of each series of readings by the different thermometers—*as observed*—should be given without any correction whatever for supposed diurnal or monthly range. All such corrections may be false, as they may be deductions from observations not in themselves trustworthy, as was the case with those from which Glaisher's Tables were constructed. But when the original observations themselves are published, every one has it in his power at any subsequent period to correct them, should trustworthy tables of correction be drawn up; and, in the meantime, he can compare them with observations made with similar instruments in other situations—a thing put out of his power to do so long as the present practice is followed.

It is to be hoped that the facts adduced have sufficed to prove that the mode of estimating the mean temperature employed by Mr Glaisher is quite inapplicable to Scotland, and only leads to erroneous results. These same facts also afford strong ground for concluding that the same will be found to be the case with the English observations. These facts seem further to prove that, in the present state of our knowledge, we cannot go far wrong in holding that to be *the true mean temperature which is the strict mean of the self-registering maximum and minimum thermometric observations*, when these instruments are of proper construction; and as this mode of estimating the mean temperature is simple, requires no correction whatever for diurnal range, is strictly comparable with similar observations made in other parts of the world, and is free from all those sources of error to which the dry-bulb thermometric observations are liable, it ought, in the meantime, *to be adopted as the true mean temperature*, and be quoted as such.

Remarks on the recent progress of Sanskrit Literature and Comparative Philology. By JOHN MUIR, D.C.L.

I propose, in this paper, to give some account of the recent progress of Sanskrit literature, and of comparative philology. As, however, these subjects have not yet attracted in this country that degree of attention which their importance demands, it may be advisable to premise some information regarding the earlier stages of their cultivation. The existence of Sanskrit, as the sacred depository of Hindu literature, was known to the early Jesuit missionaries in India; and they had not only studied that language, but also, in one instance at least, made it the vehicle of conveying religious instruction. But the effective and continuous study of this important and venerable tongue may be considered to have commenced with Sir W. Jones, who landed in Calcutta in 1783, and was the translator of the "Institutes of Manu," and of the drama of Sakuntalā, and the author of various dissertations on the mythology of the Hindus, and the affinities of that people with other nations. This study was assiduously prosecuted by Mr H. T. Colebrooke, who (in addition to various other important services) was the first to supply a tolerably complete sketch of the character and contents of the Vedas (1805); and who afterwards expounded, with remarkable accuracy, the principles of the different systems of Hindu philosophy (1823–1827). The next scholar whom we must mention as having distinguished himself in the same field, is the present Boden Professor of Sanskrit at Oxford, Mr H. H. Wilson, who published in 1819 the first, and in 1832 the second edition of his "Sanskrit and English Dictionary;" in 1827 his "Select Specimens of the Theatre of the Hindus;" in 1837, Mr Colebrooke's translation of the "Sānkhya Kārika," or "Memorial Verses of the Sānkhya Philosophy," with notes and illustrations by himself; in 1840, his translation of the "Vishṇu Purâna," a system of Hindu mythology and tradition; and in 1850–1857, three volumes of his translation of the Ṛigveda. The only other British student of Sanskrit whom it is necessary to mention here is Dr Ballantyne, Principal of the Government College at Benares, who has within the last few years made some important contributions to our knowledge of

Hindu philosophy. (See the last number of this Journal, p. 103). The study of Sanskrit was early taken up, and has long been pursued with vigour, on the continent. In 1808, F. Schlegel published his essay on the language and wisdom of the Indians. In 1823, A. W. von Schlegel published an edition and Latin version of the "Bhagavad Gītā;" and (1829–1838) two volumes of the "Rāmāyana," with a translation of the first. F. Bopp seems to have begun in 1816 the publication of his works on the grammar of the Sanskrit, and on comparative philology,—a science towards the construction of which he has since then so largely contributed. The other foreign scholars who have laboured most assiduously and fruitfully in the cultivation of Sanskrit literature, are the late Frederick Rosen, editor and translator of the First Book of the Rig Veda (1838); the lamented E. Burnouf, editor and translator of part of the "Bhāgavata Purana" (1840–1847), and the historian of Indian Buddhism; Lassen, the author of the "Indian Antiquities" (Indische Alterthumskunde, 1847–1858), an attempt to construct a continuous history of ancient India from the mass of heterogeneous data afforded by Indian literature, coins, rock and pillar inscriptions, &c.; Boehtlingk, editor of the Indian Grammar of Pānini (1839, 1840), and (in company with Roth) compiler of a Sanskrit and German Lexicon; Roth, the author of three dissertations on the Veda, and editor of the Atharva Veda, 1855–1856; Benfey, editor and translator of the Sāma Veda; Max Müller, editor of the Rig Veda (1849–1856), and author of the "History of Ancient Sanskrit Literature" (1859); Weber, editor of the White Yajur-Veda, and its appendages (1852–1859), and author of a short history of Indian literature (1852); Goldstücker, the compiler of a new Sanskrit-English Dictionary on the basis of Wilson's; Gorresio, editor of the "Rāmāyana," with an Italian version (1843–1858); Dr E. Roer, who has rendered good service by editing various Sanskrit works for the Asiatic Society of Bengal, and by publishing some translations and dissertations within the last few years; and, finally, M. Adolphe Regnier of Paris, the translator of the "Prātisākhya Sutras."

I shall now give some account of the most important labours of these scholars whose names have just been mentioned.

A new era in the department of Vedic literature may be said to date from the publication, in 1838, of the First Book of the Rig Veda, with a Latin version, by Frederick Rosen, a promising German scholar, who unhappily died in 1837, at the early age of thirty-two. His merit is to have "discovered that the character and genius of the Indian literature and language could only be completely understood by tracing them back to the earliest periods to which the Vedas belong," and to have "formed the project of endeavouring to remove the obscurity by which they are surrounded." He had intended to have prefixed to his work "a comprehensive view of the character and manners of the Hindoos in that early period;" but the completion of his work has been reserved for others.* In 1841 a translation of the Sāma Veda was published by the late Rev. Dr Stevenson, formerly of Bombay. In 1843, the text of the Sāma Veda was published by Dr Stevenson and Professor H. H. Wilson. The next writer who made any further contribution to our knowledge of the Vedas was Dr Rudolph Roth, who in 1846 published his three dissertations on the "Literature and History of the Veda" (Zur Litteratur und Geschichte des Weda; drei Abhandlungen, Stuttgart, 1846). In these short essays, a great deal of information on the contents of the different Vedas, and the Pratisakhyas or grammatical works connected with them, is contained; and some interesting historical facts relative to the relations of the priestly and regal families of India in that primitive era, and other similar points, are deduced from an examination and comparison of different hymns. In 1848–1851, appeared M. Langlois's French version of the Rig Veda, which is not very highly esteemed by German critics. Professor Benfey's edition ot the Sāma Veda, with a German translation, appeared at Leipzig in 1848. The glossary attached to this work contains many important contributions to a more accurate knowledge of the meaning of Vedic words, the most ancient extant specimens of the Indo-Germanic vocabulary. In 1852 appeared, at St Petersburg, under the authority of the Imperial Academy of Sciences, the first part of a new Lexicon, Sanskrit and German, compiled by Böhtlingk and Roth, of which two volumes, forming two-fifths of the whole work, have now been published. In this important work the first attempt on a large scale was made to apply the principles of scientific lexicography to the Sanskrit language, and to evolve etymologically and otherwise the true signification of the words peculiar to the Veda, the proper meanings of which have not in many cases been preserved by Indian tradition. The new "Dictionary, Sanskrit and English, extended and improved" from the second edition of Wilson's Dictionary, which was begun in 1856 by Professor Goldstücker single handed, and of which three numbers have now appeared, is another laborious and comprehensive effort of scientific lexicography. In his "Academical Prelections on the History of Indian Literature," Professor Albrecht Weber of Berlin supplies a condensed and comprehensive account of Sanskrit literature from the earliest period, which dwells most fully on the hymns, the Brahmanas, the Upanishads, and other similar productions connected with, or arising out of, the Vedas. The same able writer has made many interesting contributions to the history of the same early era in his "Indische Studien," and has also, as we have seen, brought out an edition of the White Yajur Veda, which he is preparing to complete, we hope at no distant period, by publishing a translation of the text of that Veda, with a partial translation of the "Satapatha Brahmana," and separate researches on the Yajurvedic ceremonies.

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* Preface to his edition of the First Book of the Rig Veda, p. vi.

The most important works, however, connected with Vedic literature, which have yet appeared, are those of Professor Max Müller of Oxford. These are his edition of the Rig Veda, and his "History of Ancient Sanskrit Literature" (1859). The Rig Veda is by far the most important of the Vedas, as it contains the most extensive collection of the oldest hymns; and while two of the other Vedas (the Yajur and the Sāma) consist, in whole or in part, of fragments extracted from the hymns for liturgical uses, the Rig Veda contains the complete hymns, arranged without reference to any such limited design, and therefore more available for historical purposes. The three quarto volumes already printed by Professor Müller contain about three-fifths of the whole collection, accompanied by an exposition by an Indian commentator of the fourteenth century, A.D. The greater part of the hymns included in these three volumes have been translated by Mr H. H. Wilson, Boden Professor of Sanskrit at Oxford. In his translations, Professor Wilson almost invariably adopts the principle of following the interpretations of the Indian commentator. But as many of the words occurring in the Vedas became obsolete in later Sanskrit, and as it is the opinion of most Oriental scholars in Germany that the true sense of many parts of the hymns has not been preserved by Indian tradition, the translation of Professor Wilson is not regarded in Germany as satisfying all the requirements of modern scholarship; and it is expected that a comparison of the Vedic words and expressions as employed in different passages, and a more exact study of their etymology, will lead by degrees to a nearer ascertainment of the sense.

In his "History of Ancient Sanskrit Literature," which had been long expected, and has but recently appeared, Professor Max Müller has presented us with a very accurate and comprehensive, though still a general, account of the Vedas, and all the works connected with and growing out of them.

The whole interval during which this entire literature was in process of formation, is divided by Professor Müller into four periods—viz., the *Chhandas* period, the *Mantra* period, the *Brāhmana* period, and the *Sûtra* period. The first period he computes to have lasted from 1200 to 1000 B.C., and during that time the most ancient of the Vedic hymns were composed. The second period lasted from 1000 to 800 B.C., when those later hymns were composed which are distinguishable from the older ones by bearing traces of the growth and development of a sacerdotal spirit and system, and by other indications of a departure from primitive simplicity and artlessness. The third period, which lasted from 800 to 600 B.C., was that of the *Brâhmanas*, or most ancient liturgical books, in which the ritual application of the hymns is prescribed with tiresome minuteness, and often with a mixture of childish allegorical interpretation. The fourth period is that of the *Sûtras*, or aphorisms, in which the ceremonial prescriptions were reduced to a more compact form, and to a more precise and scientific system.

This volume is one which every scholar and every genuine student of ancient history will peruse with the greatest pleasure. There could be no greater mistake than to suppose it the production of a mere Orientalist. There may no doubt have been, and possibly there may still exist, some Orientalists of contracted views, who attach an exaggerated importance to the particular department of research to which they have devoted their lives; though this could not be justly said of some of our best Indian philologists, such as Jones and Colebrooke. But, in fact, neither narrow nor comprehensive views are peculiar to investigators in any one department of learning. If some Orientalists have been men of a contracted mental range, the same charge may be justly brought against many students of occidental learning, who have never conceived that any light could be derived from Eastern sources to elucidate the languages, history, or mythology of the West. This class is now, however, fast dying out, and a conviction that the history of language, of religion, of civilisation, of philosophy, and of the progress of the human mind in all its developments, may receive the most valuable illustration from Oriental sources, is rapidly gaining ground among all the ablest writers in Germany and France, and, in a less degree, in our own country also. Professor Müller is one of the ablest writers of this new school. He is a scholar, who to a philosophical mind and comprehensive views unites a refined taste and a poetical imagination. Though a German, he is master of a beautiful English style. His work, like all others of a learned character, would be incomplete and unsatisfactory if it did not supply us with precise and accurate details on all branches of the subject which it professes to treat; but, while furnishing such details in a readable shape and in lucid order, it also contains much matter that is of the very highest general interest. Commencing with a statement of the objects of Sanskrit philology, especially of the light which it is calculated to throw on the earliest progress of the human mind, it proceeds to touch upon the common origin of the Indians with the Greeks, Latins, Germans, &c., and depicts in lively colours the marked contrast between the parts performed in the great drama of history by the western and the eastern branches, respectively, of the great Arian race;—between the energetic activity exhibited by the Greeks in all that concerns the outward existence of man, and the character of dreamy inaction displayed by the Indians after they had become separated from the other members of the same original stock, and had begun to develop in their new home the peculiar features of their own moral and intellectual life. The concluding chapter is certainly not inferior in value to the introductory portion of the book. In it the author treats of the most ancient hymns of the Veda, of which he adduces several curious and interesting specimens; adding, at the same time, many profound and beautiful reflections in reference to the religious and philosophical thought exhibited in these ancient productions.

Notwithstanding the important contributions already made to Indian philology, this study must yet be regarded, as Professor Müller himself remarks (p. 3), as being still in its infancy, or at least as having very much still remaining for it to effect. His work, with all its high merits, leaves much to be done for the study of the Vedas; in fact, it does not profess to offer us more than Vedic prolegomena. A minute and detailed examination and comparison of the hymns still remains to be executed. An attempt must be made to interpret them more accurately, to classify them, to fix their comparative age, to ascertain which are the most ancient and original, and which of them have borrowed those phrases which are common to them with others; to trace the history of their authors the *rishis* (or bards), their relative antiquity, and their relations to each other; to determine what ideas these *rishis* entertained of themselves—whether, originally or at a later period, they looked upon themselves as inspired; what conceptions they entertained of their gods; and whether there are any certain indications in the hymns of an older and a newer mythology (as Professor Roth supposes). Much also remains to be done in determining what are the real relations of the early Indian and Iranian (or Persisn) nations, as well as of their mythologies. We should like to have it ascertained, if possible, more nearly than has yet been done, at what era, and in what country, and on what grounds, those two branches of the Arian race (which continued to live together long after the other members of the same stock had departed to the north-west) were at length impelled to separate. We are also in want of further information as to the degree of connection which exists between the mythologies of India and of Greece. This subject was treated in Professor Müller's article on Comparative Mythology, in the Oxford Essays for 1856; and a branch of it has lately been discussed by Dr Kuhn in his "Herabkunft des feuers und des Götter-tranks" (Descent of Fire and of the Celestial Beverage).

The science of comparative philology is, as is well known, very closely connected with the study of Sanskrit. It is this ancient and venerable tongue which has supplied the wanting link for binding together the different languages of the Indo-Germanic family, and the key which most effectually unlocks all the mysteries of their structure, and of their mutual relations. I must presuppose in the reader a general acquaintance with the principles of this new science of comparative philology, founded by Bopp and other German scholars; as, for instance, the division of languages into groups or families, such as the Semitic (consisting of Hebrew, Arabic, Syriac, &c.), the Indo-Germanic (consisting of Sanskrit, Greek, Latin, Gothic, &c.), according to their respective affinities in regard (1.) to roots, and (2.) to structure—*i.e.*, the laws for the formation and inflection of words; and I shall also presuppose an acquaintance with the desults reducible from the science in regard to the mutual affinities of the nations by whom the languages composing these several groups have been respectively spoken. I shall only allude to some of the more recent labours in this department of linguistic science. A new edition of Professor Bopp's "Vergleichende Grammatik" (Comparative Grammar of the Sanskrit, Zend, Armenian, Greek, Latin, &c.), is at present in progress, and has reached the close of the second volume. A third volume will complete the work. In addition to the German "Journal of Comparative Philology," in reference especially to German, Greek, and Latin, established by Drs Aufrecht and Kuhn, a new journal, entitled "Contributions" (*Beiträge*) to the same science, in reference to the Arian, Celtic, and Slavonian tongues, has been lately commenced by Kuhn and Schleicher. The "Journal of the German and Oriental Society" also from time to time contains articles on the same subject. An interesting attempt has lately been made by M. Adolphe Pictet of Geneva, in his work entitled "Les Origines Indo-Européennes, ou les Aryas

Primitifs, Essai de Paléontologie Linguistique," to carry out in detail an idea which had been previously conceived, and to some extent realised, by Dr Kuhn of Berlin—*i.e.*, to examine the words expressive of the principal physical objects, animate and inanimate, in the different kingdoms of nature, as well as those denoting intellectual and moral conceptions, which are common to the different languages of the Indo-Germanic race, and (on the necessary assumption that the common ancestors of these different nations originally spoke one common mother-language, and inhabited one common country) to discover from these data what that original country of these primitive Arians was, and what were their physical condition and their mental characteristics at the period when they dwelt there. This idea has been carried out in part by M. Pictet, in his first volume, with great labour and ingenuity; and the result to which he is led (and which coincides with that attained by most previous investigators) is, that the primitive country of the people who were the common ancestors of the Indians, Persians, Greeks, Latins, Germans, Slavonians, and Celts, was the region lying between the Caspian Sea and mountain range of the Hindu Kush. Having completed in this volume the physical portion of his task, M. Pictet is to proceed, in a second volume, to estimate the intellectual condition of the ancient Arians, from the words expressive of that class of ideas which are common to the different languages of their descendants. The published volume of this work has formed the subject of an acute critique by Professor Albrecht Weber of Berlin, in Kuhn and Schleicher's Journal, ii. pp. 250 ff. Professor Weber asserts—and his strictures appear to be justified by the instances which he adduces,—that M. Pictet does not exhibit an acquaintance with Sanskrit sufficiently critical for the requirements of his task, and has displayed a want of scholar-like tact in failing to recognise the historical developments of that language,—defects which are exemplified in his placing words of modern formation, or ill authenticated, or figuratively employed, on the same footing with words indisputably ancient or well certified, or used in their proper and original sense, and turning them all to account indiscriminately, for the purposes of his argument. If this be a correct account of M. Pictet's procedure, it is clear that we must not regard this work as more than a contribution to our knowledge of this interesting subject. But in fact the investigations on this and other kindred topics, though diligently pursued, have but little more than begun.

While, however, such vigorous efforts have been put forth in other parts of Europe, to penetrate into the dense uncleared forest of Indian learning, and to elucidate the various problems of comparative philology, we regret that we are unable to point to any assistance which has as yet been rendered by any person resident in Scotland towards the prosecution of these interesting researches. In fact, it cannot be denied that not only oriental and comparative philology, but philology generally, has been but little cultivated in this country. And this state of things can occasion no surprise. Little encouragement has been offered by the State to the higher branches of learning, and mediocrity in scholastic acquirements has satisfied all the requisitions of the Church. Happily, however, the public mind has been awakened to a just sense of our unsatisfactory position in this respect; and this conviction, as it has led to a new law for the reorganisation of our University institutions, will also, it is to be hoped, lead to the exaction of a higher standard of learning and of culture (a standard more in consonance with the intellectual progress of the laity and the moral wants of our era) from those who seek to enter on the office of the sacred ministry, and by so doing claim to constitute themselves the spiritual, and, in some respects, intellectual leaders of their age. We trust that the new arrangements which are to be made by the University Commissioners will result in a great elevation of our intellectual standard in general, and that among other improvements they will include a provision for the efficient cultivation of those branches of learning which have formed the especial subjects of this paper—the Sanskrit language and literature, in conjunction with comparative philology.

P.S.—I find that I have omitted to advert, in the preceding sketch, to several important contributions to comparative philology of recent date, which might have been properly noticed; such as Professor M. Müller's "Last Results of the Turanian Researches," (in Bunsen's "Christianity and Mankind," vol. iii.); Pott's review of the same Dissertation in the

"Journal of the German Oriental Society," ix. 405–464; Dr Caldwell's "Comparative Grammar of the Dravidian (South Indian) Languages;" and M. Ernest Renan's "Histoire Générale et Système Comparé des langues Sémitiques, 2d edit.

Some Miscellaneous Observations on the Tadpole; on the Albumen of the Newly-laid Egg; on the Growth of Birds; their specific gravity; and on the Stomach of Fishes in relation to Digestion. By JOHN DAVY, M.D., F.R.S. Lond. and Edin.*

I shall offer a few preliminary observations on the jelly in which the ova of the frog are enveloped—a matter somewhat peculiar in its nature and remarkable in its properties, and performing, no doubt, an important part in the early stage of batrachian life.

The quantity of solid matter that this jelly contains is extremely small—less than 1· per cent; one specimen evaporated to dryness yielded ·49 per cent. It had been kept, previous to evaporation, in a moist filter, covered, to draw off what might be considered superfluous water. Another yielded only ·37 per cent; and a third even less. The dry films, on the addition of water, rapidly expand from imbibition, but they absorb less than they have lost, and consequently do not recover their original bulk. A portion that before drying weighed 40 grains, after absorption of water weighed only 10 grains.

The refractive power of the jelly differs so little from that of water, that, when immersed, the ova detached, it is hardly observable. Under the microscope, it appears perfectly transparent; but when evaporated on a glass support, so as to be moderately concentrated, granules and spindle-shaped forms become visible. By boiling in water for several hours, it is in great part dissolved—a just perceptible fibrous matter remaining. The solution is very slightly affected by infusion of nut-galls and corrosive sublimate; by both a perceptible turbidness is produced. A like effect is occasioned by nitrate of silver. A solution of this salt—12 grains to the ounce of water—occasions a slight contraction of the jelly, and its discoloration on exposure to light; now, seen under the microscope, it seems to be composed of globular masses of unequal dimensions, having somewhat the character of cells. It is acted on by the mineral acids. At first, on immersion in them, it contracts a little; it is slowly dissolved without change of colour. In the same acids heated, it dissolves rapidly; in the nitric imparting a yellow colour, in the muriatic without change of colour, in the sulphuric occasioning a brown, almost black, discoloration, with the disengagement of sulphurous acid gas. These acid solutions, when largely diluted, become slightly turbid.

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* Read before the Royal Society of Edinburgh.

Mr Brande, who was the first to examine this substance—and I am not aware that it has been examined since—was led by his experiments to the conclusion that it is a peculiar matter, and, as regards its chemical properties, intermediate "between albumen and gelatine."* From the results I have mentioned, in some particulars not entirely agreeing with his, I am disposed to view it as a variety of albumen; and, physiologically considered, the part it performs seems to be tolerably in accordance. Its uses appear to be, first, to protect the ovum, and after its hatching, to afford food to the tadpole. As regards the first, by its glairy adhesive quality, it helps to retain the ova in the water in which they have been shed, and to defend them from the attack of insects and fishes. It may also retard their freezing, and so preserve their vitality; for I have found that after having been frozen their death has ensued, denoted by their development on thawing being altogether arrested. That the jelly serves as the food of the tadpoles, is proved by its rapid disappearance after their birth from the egg. It may be useful also in preserving the ova from desiccation (the water in which they are all but failing), by its imbibing power—and this although I do not find that it retards evaporation—equal quantities of water and jelly exposed to the air having been reduced to dryness in about the same time. May not this property aid in explaining some of the statements made with the intention to prove that the frog and toad may be produced without passing through their larva stage?†

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* Philosophical Transactions for 1810, p. 219.

† See Proceedings of Royal Society, vol. vi., p. 292.

The ova of the frog in some of their properties nearly resemble those of fish. They appear to be composed chiefly of an albuminous matter and oil—the former coagulable by the admission of water, and, after coagulation, redissoluble by admixture with common salt, or with any other of the neutral salts; and further, like the ova of fish, not coagulable at a temperature of 180° or even of 212° Fahr., if not immersed in water. Their loss of vitality is denoted by their becoming of an opaque white; thus too resembling the same ova, and from the same cause, the imbibition of water.

From the observations made by Mr Higginbottom—these in opposition to those of Dr Edwards—it would appear that neither the ova nor the tadpoles of the batrachians are influenced by light;* with which, the trials I have made on those of the frog agree, as they do also with his results on the latter, showing how materially they are affected in their progress by temperature, and food, and air—a low temperature and a scanty supply of food retarding their development, and *vice versa.* The influence of air, I may remark, is well shown by placing the spawn of the frog in a deep glass vessel full of water: the ova nearest the surface will first be hatched, those lower next, and so on, many days intervening between those first and last appearing.

Mr Higginbottom found tadpoles kept in a deep cellar, the temperature of which varied from 50° to 54°, still in their larva state so late as the middle of November, or about eight months from the time of their leaving the egg; I have found some in this state so late as the 13th of February, or about eleven months from their birth. These were kept in the open air in an earthenware pan, were supplied with very little food, and with no water, except by rain. The last survivor was active when the temperature of the water was only 33°. On the 1st March it was found dead, and then its hind extremities were partially protruding. Dr Edwards, it would appear, has kept tadpoles equally long, and under the same circumstance of a very scanty supply of food.†

It is well known that the metamorphosis of the tadpole is attended with a diminution of bulk. The following results which I have obtained may be adduced in illustration, and for the further purpose of showing that, though there is a decrease of volume, there is a proportional increase of solid matter in the transition.

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* Philosophical Transactions for 1850, p. 401.

† On the Influence of the Physical Agents on Life (English Trans.), p. 52.

The ovum of a frog, carefully detached from the including jelly, weighed 0·1 gr.; dried over steam, it was reduced to 0·04 gr., or 4 per cent. solid matter.

A young tadpole, its branchial filaments and tail distinct, deprived of excess of moisture by blotting-paper, weighed 0·24 gr.; dried, was reduced to 0·06 gr., or 6 per cent. solid matter. It measured 0·40 inch in length, including its caudal fin, which was 0·24 inch long.

A tadpole, its hind feet formed, and protruding, its tail but little diminished, weighed 5·3 grs.; dried, was reduced to 0·4 gr., or 7·54 per cent.

A tadpole, its fore and hind feet formed, its tail but little diminished, weighed 2·8 gr.; dried, it was reduced to ·28, or 10 per cent. solid matter.

A young frog fully formed, its tail entirely absorbed, weighed 1·4 grs.; dried, it was reduced to ·18 gr., or 12·35 per cent. solid matter.

This proportional increase of solid matter, I find, is associated with the formation of bone. This is well shown by the result of the incineration of the tadpole, and of the young frog. In the one instance, when the animal matter is entirely consumed, the residue appears without any regular form; in the other (however young, provided it be fully formed), a distinct skeleton is obtained. And, examined chemically, I have found it to be composed chiefly of phosphate, with a little carbonate of lime. In the residual ash obtained from the tadpole, a reddish spot was observable, owing its colour to peroxide of iron, and, it may be inferred, derived from blood in the heart. There was also seen a convoluted marking, attributable, it may be conjectured, to the intestine; and in confirmation, it may be remarked, when this portion of the ash was microscopically examined, some infusorial forms were to be seen in it, which might have constituted part of the food.

From seeing the reddish spot just alluded to, I was induced to make trial of the blood corpuscles. They were incinerated on slides of glass fit for use under the microscope, exposed to heat in a crucible. The whole of the animal matter consumed, the minute residual ash, in a finely granular state, exhibited the forms of the corpuscles, with a partial reddish tinge just perceptible, some circular, some elliptical.

On the temperature which the tadpole and young frog are capable of bearing, I have made a few trials, the results of which are briefly the following:—A temperature gradually rising to 94° has been borne by tadpoles for a considerable time without loss of life, or sensible loss of activity. At about 96° or 97° their activity has diminished, and they have become torpid. In this torpid state they have sustained a temperature of 100°, not long continued; on its reduction they have become active. The less advanced in growth, the more tolerant they have proved of the influence of heat. Thus, when two tadpoles well advanced in size, and other two of smaller size, were kept in water of 100° for the space of thirty minutes, gradually cooling from this degree, the first were found motionless, and never recovered; the second, from a torpid state soon recovered their activity. At a temperature between 110° and 115°, even the youngest have been found to die, and that rapidly; in the instance of the larger, the abdomen has been seen to burst, and the intestines to protrude. The full-grown tadpole and the young frog are in a remarkably less degree tolerant of heat. A temperature so low as 88° has proved fatal to both.

The influence of a certain degree of heat, in rendering the tadpole and young frog torpid, brings to recollection what we read of the effect of tropical heat with drought on the alligator, which is said to remain in the passive condition just mentioned so long as the hot and dry season lasts.

I have also made trial of the action of salt-water on the tadpole. In a solution of common salt, of sp. gr. 10425, some lived about half an hour; in a very much weaker solution, viz. of sp. gr. 10130, the activity of others was not diminished; this after half an hour; after two hours they became languid; half an hour later they were found dead. In a still weaker solution, one of sp. gr. 10065, they lived about forty hours.

May not this effect of salt-water, and so dilute as that used in the two last trials, inferior to that of the sea, be considered as one of the causes tending to narrow the habitats of the species?

2. *On the Albumen of the Newly-laid Egg.*

The albumen of the egg of the common fowl, newly laid, has properties differing in some particulars from those of the albumen of the stale egg. One of these, and that which is best known, is the milkiness which it exhibits when dressed for the table, provided the egg be not put into water of too high a temperature, and kept there unduly long. Another is seen in the manner of coagulating.

Though the differences alluded to may be well known, I am not aware that they have been elucidated by experiment. This I mention as a reason for entering upon the inquiry,—if I may use that term in reference to a subject which to many may appear too trivial to deserve the name.

1*st*, *Of the Milkiness*.—I may premise, that if the albumen of two eggs—one newly laid, one kept for a week or more—be compared before being boiled, each will be found to be equally transparent, commonly little or no milkiness will be appreciable in either of them, proving that the milkiness which appears in the newly laid one, after being boiled, is the effect of heat. The temperature required is about 150° Fahr. After immersion in water of this degree of heat for about two minutes, the white of the newly-laid egg will be found on examination to have very much the appearance of milk, being liquid, with little or no viscidity, and mixing with water, and imparting to it, and to a large quantity, the same quality, and for a considerable time. Seen under the microscope with a one-eighth inch object-glass, the milky albumen is found to abound in granules of great minuteness, so minute, indeed, as to be only just distinctly visible, with which are intermixed a few particles of a larger size, as if aggregates of granules.

These granules, which are the cause of the milkiness, appear to be a modification of albumen. This is inferred from not being able, by treatment with ether, to extract from the milky fluid any oily or fatty matter, and from the circumstance that the granules disappear when acted on by a solution of potash.

2d, Of Coagulability.—The difference as to this property seen in comparing the white of the newly-laid egg with that of an egg kept some time, though only small, is pretty well marked. It is shown by the following trial. To two similar glass tubes, equal portions of the white of an egg laid the same day, October 26, which may be called No. 1, and of one laid on the 23d April, No. 2, were introduced, and were subjected to different degrees of temperature, by immersion in heated water, for the space of two minutes.

At 140° no change was seen in either.

At 150° falling to 145°, No. 1 had become in great part opaque, *i.e.* of milky opacity; No. 2 only at the surface, and very slightly.

At 162° falling to 159°, No. 1 showed a milky opacity throughout—had lost little of its fluidity, as shown by being very easily poured out; No. 2 was in part so coaguluted, as to admit of inversion; a gelatinous coagulum adhered to the inner surface of the tube.

The results of another trial were the following:—

At 155° falling to 145°, about half of No. 1 acquired a milky opacity—its upper portion; No. 2 had become so to less than the extent of one quarter.

At 166° falling to 160°, No. 1 had become opaque throughout, retaining its fluidity. No. 2 had also become opaque; it had lost its fluidity—at least it did not flow when the tube was inverted.

At 175° falling to 168°, No. 1 was still fluid.

At 195° falling to 185°, No. 1 was coagulated throughout; the coagulum moderately soft. These results seem to show that the white of the newly-laid egg is more readily affected by heat of a certain temperature than that of an egg exposed some time to the air, as indicated by the appearance of milkiness it exhibits, and yet that, within a certain range of temperature, the amount of coagulation or the degree of firmness is less. Many other trials which I have made have given similar results. I shall confine myself to the mention of one.

A portion of the white of a newly-laid egg was poured into a tube, and over it a portion of one that had been laid seven days; they were kept apart by a thin pledget of cotton. At 162° falling to 152°, an opacity appeared at the surface of both at the same time; but it spread through No. 1 more rapidly than through No. 2. The former was opaque throughout, when the latter was transparent except at the surface. Kept in water till it had fallen to 148°, No. 2 also had become opaque throughout; it was so firmly coagulated that it had to be broken up before it could be removed from the tube. No. 1 on the contrary, was of very soft consistence, and most easily removed. The trial was repeated, reversing the position of the two, the white of the newly-laid egg over that of the older. The results were similar. The former was soonest affected; the latter, in the end, was most firmly coagulated.

That the difference of qualities which I have described is owing to exposure to, and the action of atmospheric air, can hardly be doubted.* The proofs seem to be sufficiently clear. The newly-laid egg contains little or no air;† and if atmospheric air be excluded, its absorption prevented, as by lubricating the shell with oil or any oleaginous matter, the albumen retains for a considerable time the qualities of the newly-laid egg. This is a fact well known to dealers in eggs.

The exact time required for the change to take place, owing to the absorption of air, I cannot exactly say; it varies, I believe in some measure, according to the season—a shorter time in winter being required than in summer; the egg in the former season, owing to lower atmospheric temperature, contracting more in bulk, as regards its substance, than in the former. A very few days, as from five to six at furthest, seem to be sufficient. In April I have observed the milkiness on the fourth, but not later. On the contrary, if air be excluded, I do not know how long the quality of the newly-laid egg may not be preserved. I have found an egg laid in the month of April, and then smeared with butter, hardly appreciably changed at the end of six months.

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* May not the disappearance of the curd which is seen in the salmon when dressed fresh from the sea, the clean fish of the angler, be owing to the same cause—viz. the absorption of oxygen on being kept exposed to the air, and the liquefaction of the curdy matter, a consequence of that absorption—a liquefaction similar to that which the fibrin of the blood undergoes from the action of oxygen.

† See "Anatomical and Physiological Researches," by the author; vol. ii. p. 222.

Nor can I speak with any exactness respecting the amount of air, of oxygen absorbed, or of other alterations that may be effected in the composition of the egg by its action. All that I have yet ascertained is, that with the absorption of oxygen in the instance of the stale egg, carbonic acid is formed and ammonia, and the colour of the albumen is darkened, it becoming of a light brownish yellow, and at the same time acquiring a smell somewhat unpleasant, and a taste, as is well known, not agreeable. The putrefactive process, I believe, does not take place, however long the egg may be kept, unless there be some admixture of the yolk and white.

In conclusion, I may add that though I have confined myself in the account of the foregoing observations to the albumen of the egg of the common fowl, the trials I have made have been extended to that of the egg of the duck, the turkey, and guinea-fowl, and that the results have been similar. Slight variations, indeed, have been observed, but not, I think, greater than have been witnessed in experimenting upon different eggs from the same fowl, or upon eggs of different fowls of the same kind.

3. *On the Growth of Birds.*

The observations I have to offer on this subject have been confined chiefly to four birds—the martin, the common fowl, the turkey, and goose; the first feeding on insects, and never but on the wing; the last on vegetables, subsisting chiefly on grass; the barn-door fowl and turkey using a mixed diet of seeds and insects.

1. *Of the Martin* (*Hirundo urbica*).—The young of this bird, I am led to infer, is capable of taking flight in from fifteen to twenty days from the time of hatching; for in about this time, reckoning from the day that I have found the broken egg-shells thrown out of the nest, I have seen the young birds first on the wing.

On the 16th of July, a young martin taken from its nest, not in full feather, weighed 346 grains; opened, much fat was found within its abdomen. Thoroughly dried on a steam-bath, its weight was reduced to 112 grains.

On the 9th of July, a young martin shot on the wing, supposed to have taken flight for the first time that morning, weighed 303 grains; thoroughly dried, it was reduced to 110 grains.

On the 4th of August, a young martin, which it was believed had left its nest many days, shot on the wing, weighed 270 grains; some flies were found in its stomach. There was less fat within its abdomen than in that of the imperfectly-fledged nestling. By thorough drying it was reduced to 110 grains.

On the 7th August an old martin was shot. It weighed 273 grains. No fat was found within its abdomen. By thorough drying it was reduced to 105 grains.

2. *Of the Common Fowl.*—The average weight of six eggs from a hen of the Dorking breed was 986 grains; the average weight of the same number of chickens, the produce of these eggs, ascertained a few hours after hatching, was 737 grains. One of this brood, when a week old, had increased in weight to 950 grains. Two of the same brood, a cock and a hen, when three months old, had increased, the former to 3 lbs., the latter to 2¾ lbs.

By a gentleman who has paid much attention to poultry, I am informed that in Yorkshire the average weight of barn-door fowls, hatched in spring and killed about Christmas, is from 6 to 8 lbs.: the difference chiefly owing to the sex; the males the heaviest.

3. *The Turkey.*—I have only one trial, the result of which I can give, on the growth of this bird. I am indebted for it to a friend, on whose accuracy I can depend. A young Turkey, from an egg which before incubation weighed 2¾ oz., on the day it was hatched (including the empty shell), weighed 1¾ oz.; when a fortnight old, it had increased in weight to 3¾ oz.; when four months, to 10 lbs. It was one of a flock allowed to roam the woods; it had not been put up to fatten, and, as I am informed, was only moderately fed.

From the gentleman before mentioned, I learn that the weight of the Turkey in Yorkshire at Christmas varies from 16 lbs. to 25 lbs.

4. *The Goose.*—The eggs of this bird vary greatly in weight—more than any others which I have tried. Thus, two taken from the nest at the same time differed as much as 191 grains—one weighing 2920 grains, or about 6·08 oz., the other 2729 grains, or about 5·66 oz. A gosling hatched on the 26th April, on the 30th of that month weighed 1987 grains, or 4·14 oz.; another hatched on the 17th of the same month, tried on the same day as the first, weighed 5579 grains, or 11·60 oz. The following results I am indebted for to an intelligent farm-servant: An egg, a few days after it had been laid, weighed 7 oz.; the gosling, the same day that it was hatched, weighed 4 oz.; after four days it had increased in weight to 6 oz.; after thirty-four days, to 6 lbs.; after sixty-eight days, to 7 lbs. My Yorkshire correspondent estimates the weight of the goose at Christmas at from 12 lbs. to 14 lbs.

These few examples may suffice to show how rapid is the growth of birds. Comparing the growth of the swallow fed by the parent birds, with that of the young of the turkey common fowl, and goose, which have to find their food, a marked difference is observable—the growth of the one being so much more rapid than that of the other; and it can hardly be doubted that the same will be found to be the case more or less generally. Again, if we compare the eggs of those birds which feed their young till they are capable of taking wing, with the eggs of other birds the young of which, so soon as they are hatched, have to provide for themselves, a difference also will be found, the eggs of the latter being, I believe, without exception, proportionally larger. How little, as regards size, does the egg of the eagle differ from that of the goose; how very small is the egg of the cuckoo compared with that of the partridge—the latter birds differing but little in size. And is there not design in this as well as in the different degrees of rapidity of growth? Is not the gosling, during the first days of its existence after leaving the egg, more dependent for nourishment on the residual included yolk than the eaglet? And is it not so in other instances?*

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* The subject of the size of the egg in relation to the size of the parent bird

In the mammalia there appears to be a relation between the period of growth, that required for the attainment of their maximum power, and their duration of life ; but in birds this relation seems to be entirely set aside. And is it not because a rapid attainment of their full power is essential to their existence ? This is strikingly the case in the instance of the swallow ; and in other birds we witness it in degree according to their habits and wants. An advocate for final causes might here find ample argument in support of his doctrine. If the means of this rapid growth be considered, I believe they may be referred to two things mainly—viz., abundance of food, and activity of the digestive organs of the young birds, other circumstances aiding. Take the instance of the martin, the growth of which is so rapid, that in the short space of a fortnight or three weeks it acquires a bulk and weight exceeding those of the parent bird. Those who have watched a swallow's nest cannot but have been struck by the wonderful industry and assiduity displayed by the old birds in supplying their brood with insect-food ; and the quantity of droppings from the nest, evacuated by the young birds is hardly less remarkable ; in twenty-four hours as many as forty-one have been counted, which had fallen on the flags below. These, collected and dried, weighed 70 grains ; on examination they were found to be partly fœcal, the undigested remains of insects, and partly urinary ; the latter consisting chiefly of lithate of ammonia, with a very little urea. In the other birds of which notice has been taken, the same conditions as to digestive power and the supply of food exist, varying only in degree. And the same, we believe, may be

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is an interesting matter, and well adapted for inquiry. Probably, were a strict comparison made, a variety of circumstances would be found to have an influence in regulating the relation,—circumstances chiefly referrible to the manner of feeding the young birds, as alluded to above—and the supply of food, whether scant or abundant. By way of illustration, I would ask, may not the eggs of the missel-thrush be so large, having to be hatched early in the spring ? May not those of the turkey be smaller than those of the goose, their time of hatching being considerably later ? May not those of the Cochin-China variety of the common fowl be smaller than those of any of our northern varieties—the former being the breed of a warm climate, where food, it may be inferred, is always abundant.

said of birds generally, whether their young are sustained for a time by the parent birds or have to find their own sustenance, the breeding season of all of them, and the spots selected for breeding being suitable—the one a time of plenty, the other affording the kind of food best adapted to the wants of the several species. In the other examples, as in the instance of the swallow, the activity of digestion is also confirmed by the copiousness of the evacuations; the fœcal, (as in that), consisting chiefly of matter of an indigestible kind, from which all that is nourishing has probably been extracted,—the urinary chiefly of lithate of ammonia. I have alluded to aiding circumstances: these, I believe, are to be found in some of the peculiarities of organisation of birds and their functions, promoting a high temperature, with comparatively a small waste of matter in respiration.* A powerful heart, a quick circulation, richness of blood in red corpuscles, cutaneous and mucous tissues of little activity, may be mentioned as some of these; and as resulting from these, little need of fluid injesta and little loss of fluid, either cutaneously or by the action of the kidneys and intestines.

4. *On the Specific Gravity of Birds.*

The experiments I have made on this subject have been limited to the following birds,—the martin, water-ouzel, snipe, wood-owl, merlin-hawk, and wren.

The trials made have been of two kinds—one on the birds after the removal of their feathers, to secure which they were skinned; the other with their feathers on. I shall notice the former first.

The specific gravity of a martin (*Hirundo urbica*) was found to be as nearly as possible that of the water in which it was weighed. Its cylindrical bones contained no air, but a colourless marrow.

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* According to the experiments of Messrs Allen and Pepys (Phil. Trans. for 1819 and 1829) in the instance of the pigeon and Guinea-pig, allowing for difference of bulk, the quantity of carbon consumed by each, indicated by the carbonic acid produced in respiration, varies but little—the bird, per minute, producing ·5 cubic inch; the other, ·62 inch; the volume of the former 28 cubic inches, of the latter 39 cubic inches.

The specific gravity of a snipe (*Scolopax gallinago*) was 1038. Its bones were destitute of air. The marrow in the long bones of the legs and wings was reddish-yellow.

The specific gravity of a water-ouzel (*Turdus cinclus*) was 1200. Its long bones contained a reddish marrow.

The specific gravity of a wood-owl (*Strix stridula*) was a little less than that of water; the body, which was a little heavier, having been buoyed up by the head, that being of less specific gravity, owing to the cellular construction of its bones.

The specific gravity of a merlin-hawk, its head cut off, was about the same as that of water; its head was a little lighter, from its cellular structure; air was found in its long bones, the femoral and humeral; and a yellowish fat in its fibiæ and radii. Notwithstanding the presence of air and marrow in these bones, the wings and legs, deprived of their feathers, sunk in water.

The specific gravity of a wren (*Motacilla traglodytes*) was 1017.

The results of the trials on these birds with their feathers on, have been, as might have been expected, less precise. I need hardly observe, that they all floated in water, or that as their feathers became wet on submersion (submerged by a weight attached), the more they became wet the more air was detached, and the more the specific gravity was increased. I may mention an instance, that of the wren: when first submerged, its specific gravity was found to be 0·890; after being under water twelve hours, it had increased to 0·960.

The least unsatisfactory trial I have made has been that on the water-ouzel. Its specific gravity was found to be 0·724. When under water, few air-bubbles escaped from its feathers, owing probably to their resisting wetting, from the oil with which they are pruned, *that* being abundantly supplied by the large oil-gland with which this bird is provided.

The bird the specific gravity of which I have found lowest—if I may so speak of an approximate result—has been the merlin-hawk; it was 0·570.

In conclusion, it may be remarked, that, judging from the foregoing results, the specific gravity of the body of birds is concerned but in a very subordinate manner, with their aptitude for aërial locomotion. That aptitude seems to depend on other circumstances, such as the great lightness of their feathers, owing to the air which they contain; the little tendency of water to adhere to them, even when exposed to rain; their form and arrangement, so admirably adapted for the purpose of impulse—the high temperature of the body expanding the contained air, and the immensely powerful muscles, the pectoral, belonging to the wings. Is not the power of flight of each species in a great measure proportional to these conditions?

5. *On the Stomach of Fish in Relation to Digestion.*

The observations I have to offer on this subject have been made chiefly when on angling excursions. They were begun in consequence of having my curiosity excited by so often finding the stomach of the salmon and sea-trout perfectly empty of food. This, indeed, is a well-known circumstance, giving rise to the popular notion that these fish, after leaving the sea, abstain altogether from food.

This empty state of stomach led to the question, Is it accompanied or not by the presence of the gastric juice? To endeavour to find an answer I employed test-papers, taking it for granted, that if any gastric juice were present, it would be denoted by an acid reaction. For the sake of precision, I shall relate a few of the many instances in which I used this test, beginning with the salmon (*Salmo salar*) and the sea-trout (*Salmo trutta*).

On the 24th August, four salmon taken by net in the sea, at the mouth of the Crede, one of the salmon-rivers of Lewis, were opened, and about three hours after their capture. The stomach of each was empty, that is, contained no solid food, nor indeed any liquid of any kind, merely a little adhering mucus. No effect was produced on litmus paper, applied to different parts of its lining membrane.

The stomachs of other four, taken in the sea at the same place on the 26th of the same month, were also found empty. Tested by blue litmus paper, no effect was perceptible; tested by reddened paper, a very slight alkaline reaction was indicated.

On the 27th August, the 3d and 6th of September, the stomachs of two salmon and of one grilse, all taken with the fly in fresh water at Loch Morsgael, also in Lewis, were opened immediately after their capture. As in the preceding instances, they were found empty; and tested in the same manner, they gave like results.

With the young of the salmon, before their descent to the sea, the parr and the smolt, the results of all the trials I have made have been different. These fish, it is well known, are greedy feeders. I have always found food in their stomach, chiefly flies, and the stomach has always exhibited a distinct acid reaction. The fish I tried were most of them from the Teith and the Welsh Wye, and the examination was made as soon as they were taken from the water.

The number of sea-trout which I have opened has been large, all of them taken with the fly in fresh water, and chiefly in the lakes of Lewis and Harris, which I had the privilege of fishing through the kindness of Sir James Matheson, the proprietor of the former, and of Lord Hill, the lessee of the latter. In the great majority of instances the results were the same as those afforded by the salmon, the stomachs being found empty, and without effect on test-paper. In a few, the results were different; these may require notice. Of twenty taken in Harris on the same day, in Loch Vosmet, September 8th, three had food in their stomach, believed to be small trout (they were partially digested, so that their character was obscure), an acid reaction was distinct in each. Of forty-two taken in Loch Morsgael, August 28th, the same reaction was witnessed in the stomach of two; a few minute flies, and these only, were found in each. In one instance only have I ever noticed a reaction of this kind in the sea-trout with an empty stomach, and that was in one of the forty-two just mentioned.

I shall now pass to the common trout (*Salmo fario*), on which I have made very many observations, and these at different seasons of the year, and on individuals of different sizes, from the brook-trout of two or three ounces to the lake-trout of as many pounds. The results have been remarkably uniform. In the great majority of instances, food has been in the stomach, and in all a distinct acid reaction has been witnessed. On the contrary, in the very few in which it has been found empty, that reaction did not take place; the lining membrane was either neutral, or showed a very slight alkaline reaction.

On the charr (*Salmo umbla*) and the grayling (*Thymullus vulgaris*), the former taken in Windermere, the latter in the Wye and the Lugg, the observations I have made have been few. As the results accord with those already described, it may suffice to mention, that when food was found in the stomach an acid reaction was detected, and *vice versa*.

On sea-fish, the trials I have made have been fewer still; they have been limited to the dog-fish (*Scyllium canicula*) and the haddock (*Morhua æglefinus*). The trial with the dog-fish (it was a solitary one) was made in June in Sutherland. The fish, before it was opened, had been taken over twelve hours. In its stomach there was a considerable quantity of food in a pultaceous state; its acid reaction was decided. Of five haddocks examined, food was found in the stomachs of four; it consisted of pultaceous gritty matter, with which in one instance were mixed broken spines, like those of the aphrodita; in the others, the remains of crabs and some bivalve shells, the latter little altered. In each, the reaction was alkaline. This, I apprehend, was no more than might have been expected, considering the presence of carbonate of lime in the spines and in the shells, with the additional circumstance that many hours had intervened between the taking of the fish and the experiments made on them. To endeavour to ascertain whether any free acid had been secreted by the stomach, the contents of one were subjected to chemical examination, and with positive results, chloride of lime being detected. In the instance in which the stomach was found empty, a very slight acid reaction was just perceptible.

Though my observations have been principally directed to the stomach of the fish I have examined, they have not been entirely confined to that organ; in many instances they have been extended to the intestines. In all these, their contents have been found, as might be expected, invariably alkaline; and this whether the part tested was near the pylorus or distant from it. The same alkaline reaction was witnessed in the fluid, the secretion of the *appendices pyloricæ*—the test-paper being applied to any one of those tubes after having been divided transversely, when a little liquid commonly exuded.

In connection with these observations, I have made a few on a subject nearly allied to them, viz. the action of the gastric juice, and of the other fluids supposed to be concerned in digestion after death, on the secreting organs and parts adjoining them,—an action first observed and described by John Hunter in the instance of the human stomach.

In the common trout, the stomach of which is so rarely without food or without a free acid, indications of the action in question must be familiar to every angler in the habit of opening the fish he takes. According to what I have observed, several hours commonly intervene between the capture of the fish and the witnessing the effects, varying as to time with the temperature of the air and other circumstances of a less appreciable kind. The organs or parts most liable to suffer I have found to be not the stomach itself, but the parieties of the ribs on the side contiguous to the *appendices pyloricæ* and the upper portion of the intestinal canal. These bones have been seen bare and projecting internally, with softening, and often a breach of the intestine, and yet the stomach, at the time, has been little changed—softened only in a slight degree. The only instances that I can recall to mind of its having been partially dissolved and ruptured have been of young fish, especially the young of the salmon, the parr, taken at a season when they were feeding greedily.

These remarks on the trout are applicable to the charr and the grayling; in fish of each kind I have witnessed effects similar to the preceding.

The observations I have made on the salmon and sea trout, in relation to this action, have been fewer than I could wish; and it has so happened that they have been confined to those the stomachs of which were empty, and showed no acid reaction. In all but one, no *post mortem* effect was perceived; it was one of seven sea trout taken on the 10th September, and opened on the following day. In this exceptional one, the bones contiguous to the *appendices pyloricæ* were partially laid bare, as if from solution of their covering. Neither in the stomach of this fish nor of the other six was there any food found, or any traces of acidity. I may mention the results of a comparative experiment made with the common trout and the sea trout, showing the marked difference of effect in the two instances. The subjects of the trial were a trout of the common kind, from the river Rothay, of about half a pound, and a sea trout from Maryport of three pounds. After a ligature had been applied to the gullet and also to the lower portion of the intestine of each, the viscera of both were taken out, and placed in a glass vessel and covered. The temperature of the air was little variable, about 65°. After twenty-four hours an examination was made. In the stomach of the common trout, pultaceous food was found with an acid reaction; its coats were but slightly softened; the intestine was reduced to a shreddy state, most remarkable in its upper portion, and its contents had escaped; these showed an alkaline reaction. The stomach of the sea trout was empty, with the exception of a little adhering milkwhite mucus, as were also the intestines, with the exception of a little yellow slime. The stomach was neutral; the intestine slightly alkaline. Neither exhibited any appearance of softening.

As some of the conclusions deducible from the preceding observations, I would beg to submit the following for consideration:—

1. That the gastric juice, and probably the other fluids concerned in the function of digestion in fishes, are not secreted till the secreting organs are stimulated by the presence of food—a conclusion in harmony with a pretty general physiological law, and in accordance with what has been best ascertained respecting the gastric juice in other animals.

2. The probability that the gastric fluid—a fluid with an acid reaction—is less potent in the instance of fishes as a solvent than the alkaline fluid of the *appendices pyloricæ;* and that even as regards the gastric fluid, its acidity is not essential to it, as its action does not appear to be arrested when it is neutralised by the presence of articles of food abounding in carbonate of lime.

Lastly, as a corollary from the first, may it not be inferred that the migratory species of the salmonidæ, such as the salmon and sea trout, which attain their growth and become in high condition in the sea, there abundantly feeding and accumulating adipose matter, though not always abstaining in fresh water, which they enter for the purpose of breeding, are at least capable of long abstinence there without materially suffering? And may not this be owing to none of their secretions or excretions, with the exception of the milt of the male and the roe of the female, being of an exhausting kind? And, further, owing to the empty and collapsed state of the stomach and intestines, are they not, when captured, less subject to putrefaction, and thus better adapted to become the food of man?

REVIEWS AND NOTICES OF BOOKS.

The Plant-Covering of the Earth; a Popular Exhibition of Botanical Geography for Well-informed Lovers of Nature, composed from the Newest Sources. By LUDWIG RUDOLPH.*

Die Pflanzendecke der Erde Populäre Darstellung der Pflanzengeographie für gebildete Naturfreunde nach den neuesten und besten Quellen zasammengestellt und bearbeitet VON LUDWIG RUDOLPH, *Zweite vermehrte Ausgabe.* Berlin: Nicolaische Verlagshandlung (G. Parthey) 1859.

Geographical botany, phyto-geography, or plant-geography, is the systematic arrangement of all facts connected with the mutual action and reaction between the vegetable covering of the globe and meteorological agencies, together with the modifications effected by man for the purposes of agriculture and horticulture, so as to affect the aspects of nature on a large scale. Hence it appears, that plant-geography is an important part of physical geography farther developed on account of its influence upon the weal, the woe, and the enjoyment of man.

If we attempt to assign to phyto-geography its right position in relation to the totality of sciences, we are led to the following considerations:—

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* This Review has been transmitted by a foreign correspondent, and hence there is some peculiarity in the idiom.—*Ed. Phil. Jour.*

Science is the organic system of cognate truths, and philosophy is the organic system of all sciences. Hence it appears, that philosophy, strictly so called, cannot logically be co-ordinated to other sciences, since they are all contained in the idea of philosophy, to which they must be logically subordinated. This subordination may be effected in various ways, and it may assume different forms, *e.g.*, the following:—

All sciences are either *formal* or *material:* formal, if they consist entirely in the right conformation of thought; material, if they require, in addition, the correct observation of facts. The formal sciences do not contain the ideas of any given substance, matter, existence, or facts placed externally to the thinking mind; consequently the formal sciences may be called in preference *demonstrative* sciences, or sciences of hypothetical necessity, because they show that if one truth is admitted, other truths must necessarily follow. Logic and mathematics are formal sciences. The fundamental idea of logic is mental congruity, and the fundamental idea of mathematics is quantity, either numerical or continuous. Numerical quantity is the fundamental idea of arithmetic, and continuous quantity that of geometry.

In contradistinction to the formal sciences are the material-observing, or *monstrative* sciences, in which all demonstration is more or less based upon stubborn facts. These facts may be either transitory or continuous. Transitory facts are the basis of political history and biography, which never relate two facts in every respect alike. Natural history is based upon continuous facts, because it relates what continuously may be observed.

Political history and biography are related to natural history and the description of natural objects, as arithmetic to geometry. In history and biography, the dates are numerical. So in arithmetic the quantities are numerical. In natural history the dates are continuous; and so are the quantities in geometry. The events to which natural history refer are not regarded as past, but as continually present.

Natural history, in its widest sense, embraces the universe, and may be divided, according to its objects, into the branch relating to the cosmic bodies above the earth, *i. e.*, astronomy, and into the branch relating to the earth, *i. e.*, geography. Both branches may be divided into the observing and the calculating, which latter arises from the application of mathematical considerations to either astronomical or geographical observation. Geography may again be subdivided according to its objects, which may be inorganic (mineral), or organic (animal or vegetable). In this case we have phyto-geography, describing the plant-covering of the globe in its largest features.

In plant-geography we expect to find the following materials: *First*, A description of those plants which, by their massive forms, or by the frequency of their occurrence, give character to the countries; and *Second*, A description of the countries themselves, according to their botanical characteristics. As to the plants, they may be either of spontaneous growth, or spread by the cultivation of man.

Those of spontaneous growth are, for example, the forest-trees, with large leaves; the myrtaceous and other evergreens; the coniferous trees; the heaths, mimosas, ferns, palms, agaves, bromeliaceæ, pandanus, musæ, cannæ, gramineæ, cactus, liliaceæ, climbers, loranthaceæ, pothos, orchis, mosses, and lichens.

The cultivated plants to be considered are those which furnish food, raiment, and luxuries: plants of nourishment are *Cerealia*, such as wheat, rye, barley, oats, rice, maize, sorghum, panicum or millet, and buckwheat. Or tubers and roots, as mangold, potatoes, arums, turnips, carrots, cassava, batatas, yams. Or fruit-trees, like the breadfruit, coco-nut, date-palm, sago-palm, oil-palm, olive, mulberry, chestnut, and many others. Or plants yielding luxuries and medicines, as the vine, sugar-cane, coffee, tea, pepper, indigo, tobacco, opium-poppy, catechu, and gambir. Or raiment, like flax and cotton.

In describing the physiognomy of the countries, the following botanical zones have been adopted: 1. The equatorial zone; 2. The tropical zones, north and south; 3. The sub-tropical, north and south; 4. The warm temperate; 5. The cold temperate; 6. The sub-arctic; 7. The arctic; 8. The polar zones. These divisions are to be subdivided according to the various quarters of the globe, Europe, Asia, Africa, America, and Australia, Mr Rudolph has followed the plan here detailed.

Linnæus, in his *Flora Lapponica*, 1737; his *Flora Suecica*, 1745; and in his *Coloniæ Plantarum*, *Amœnitates Academicæ*, vol. viii., p. 1; and Gmelin, in the preface to his *Flora Sibirica*, may be considered the first authors before Humboldt, who, by their observations, introduced, or at least foreshadowed, the study of phyto-geography. Mr Rudolph does not mention these early originators of the eighteenth century.

But the merits of Alexander von Humboldt are so great, that they need not to be enhanced by concealing or forgetting what Carolus a Linne taught half a century before him, viz.:—Sapientissimus creator, qui globum convestivit terraqueum plantis diversissimis, iisque prope modum innumeris, has pro suâ etiam stupendâ ac admirabili œconomiâ, modo usque singulari disposuit, ut unaquæque amœnissimæ hujus cohortis in suâ tanquam patriâ habitaret, loco ipsi adsignato. Sic nimirum Indiæ plurimas acceperunt Palmas; temperati cœli regiones Herbas inprimis; septentrionales moderatæ Graminum multitudinem; frigidæ

Muscos et Algas itemque Arbores Coniferas, et denique America Filices. Idem summum providumque Numen hæc quoque plantarum genera in dicta distribuit climata, pro variâ soli naturâ, in Alpibus montibus, campis, pratis, agris, paludibus, aquis, etc., quo in suo quodque natali loco lætius cresceret et floreret. Qui veram cunque et solidam plantarum scientiam aucupatur, patriam ipsarum ac sedem cujusque propriam, haud sane ignorabit. Sic qui in communi vitâ, in usum œconomicum illas vel medicum convertere velit, ubicunque gentium et locorum habitent, probe sciat, necesse est, etc.

Humboldt's "Aspects of Nature," ("Ansichten der Natur,") and his "Essai sur la Géographie des Plantes, accompagné d'un Tableau Physique des Regions Equinoxiales, Paris, 1807," gave such an impulse to botanical geography and geographical botany, that he is usually considered to be the founder of this department of science, although he was not the first author who wrote on this subject. Humboldt's "Essai" has been translated into English and other European languages; and its favourable influence may be traced in almost all works on physical geography subsequently published—such as Schouw's "Grandzüge einer Allgemeinen Pflanzengeographie," Berlin, 1823; Beilschmied's "Pflanzengeographie," Breslau, 1831; Meyen's "Grundriss der Pflanzengeographie," Berlin, 1836; Grisebach's "Berichte über die Leistungen in der Pflanzengeographie," Berlin, 1844–56.

Rudolph refers to these works as his sources of information; but he has overlooked the most recent and the most important publication on plant-geography—viz., the "Geographie Botanique Raisonée, ou Exposition des Faits Principales et des Lois concernant la Distribution Géographique des Plantes de l'Epoque Actuelle. Par M. Alph. Decandolle." Two volumes. Paris, 1855.

Plant-geography having been established by such works as we have quoted, it followed, as a natural consequence, that other books were produced in order to popularise the new science, so as to introduce it into schools, and to render it attainable in private life without much preparatory study. This was the object of several popular lectures delivered and published by Schleiden. The same popularising aim predominates in the following publication by the father of a lamented traveller: Vogel's "Naturbilder," Leipzig, 1846; and in Schouw's "Die Erde, die Pflanzen und der Mensch," Leipzig, 1851; and in the "Atlas der Pflanzen-geographie," von L. Rudolph. Berlin, 1852. The work at present under review is the text-book explaining this Atlas which the author published seven years earlier, aiming to popularise the science of geographical botany. Mr Ludwig Rudolph saw that even in the better schools, numerous particulars are taught without showing their relation to a general unity: "Es fehlt unserm Unterricht zu oft die Beziehung des Einzelnen auf das

Ganze, die Erweiterung der Aussicht die hinter dem vorliegenden Gegenstande der Betrachtung die ganze Fülle ähnlicher Erscheinungen ahnen läszt, die dem Gemüthe Befriedigung, der Phantasie Nahrung giebt und den Eifer zum Weiterforschen anregt." This defect is in some degree remedied by geographical botany, which looks upon the whole vegetative covering of our globe as being one connected whole. In doing so, geographical botany directs our attention especially to those plants which give to a whole country its characteristic physiognomy, leaving to systematic botany the office of arranging plants laboriously in classes, subclasses, orders, families, genera, species, and varieties. In systematic botany the analytical, and in phyto-geography the synthetical, efforts predominate. However, let it be remembered, that analysis and synthesis can be kept widely asunder only in the abstract, and that in reality the phyto-geographer must be trained by systematic botany for accurately observing plants.

The pleasure which we derive from the contemplation of the vegetable creation in forests, fields, and meadows, is, in its indefinitiveness, similar to that from instrumental music. The vague sensations created by instrumental music assume more definiteness when harmonising vocal music is added. So also the pleasurable but vague sensations arising from the contemplation of nature are raised to definiteness when the beholder becomes conscious of their importance by the interpretations offered in botanical geography. Systematic botany may be compared with acoustics, giving an account of all sounds. A merely sensuous delight in forests, fields, and meadows is like revelling in instrumental sounds, the pleasure in which is much augmented by a knowledge of thorough bass, which enables us to adapt the human voice so as to indicate the meaning, aim, and intention of the sounds with which it acoustically and mentally harmonises. Nature speaks to us and to all men, but it gives an uncertain sound, and becomes intelligible only when man interprets it in words. Nature, like music, requires the mouth of man to render it fully enjoyable, by giving utterance to its meaning.

Systematic botany, in classifying plants, separates the groups in which they naturally grow, and severs from the isolated plant its organs of fructification, which again are pulled to pieces in order to submit minute portions to the microscope, aiming to establish divisions and subdivisions of the *Systema Plantarum.* But the plant-geographer does not cut down any plant, and does not pull flowers asunder, because he has not to systematise, but to consider the influence of climate upon plants generally, and also the reaction of plants upon climate. Systematic botany has to consider chiefly some minute portions of the sexual organs of propagation, while the plant-geographer attends more to the massive portions of plants giving character to the scenery of countries; consequently the organs of nutrition and preservation are to him of more importance than those of generation, unless taken in the mass. Systematic botany endeavours to know 200,000 species of plants, which are all nearly of the same systematic importance. But a small number from this host are selected by the plant-geographer—viz., those which sensibly affect the weal and woe, the comfort and enjoyment of man—and for which he has never to search, because they stare him boldly in the face, being so massive, either in themselves or by their aggregation, that they cannot be overlooked.

Plant-geography takes its isothermal, isotheral, and isochimenal lines from physical geography, but considers them, as also the events explained in meteorology, in their effects upon vegetation.

We highly approve of the manner in which Mr Rudolph advocates the importance of phyto-geographical studies, and of the correct conception of their general aim and limits; but in the execution of the work, we find some defects and errors which should not have been transmitted to a second edition. Having been honoured by the King of Prussia with a gold medal for his "Phyto-geographical Atlas," certainly the printed pages of the "Text Book" should not be in contradiction with themselves and with known facts.

On page 446, we find the words of the text, "*Die chilenische araucaria*" at the foot of the page explained by "*Araucaria excelsa.*" But this is a mistake; because the *A. excelsa* comes from Norfolk Island, and the *A. imbricata* from Chili. It seems that the word "*Aronswurzeln,*" on page 432, means *Arrowroot.* The author should have translated "*Pfeilwurzeln,*" if he would translate a name almost received into German; but to put "*Aron*" instead of "*Arrow,*" creates confusion. A more important error than mere misnomers occurs on page 40. L. Rudolph repeats, undoubtedly on the authority of some scientific works—"Die Europæische Zwergpalme Chamærops humilis, die in der Barbarei als dichtes Gebüsch erscheint . . . . . findet sich am ganzen Mittelmeer." This is a geographical mistake. *Chamærops humilis*, which abounds on the western shores of the Mediterranean, on the Barbary coast, and in Spain, is, for causes which plänt-geography should endeavour to discover, on the eastern shores either entirely wanting, or very scarce. The writer of this review has not met with it in Egypt, nor in Palestine, nor in Syria, nor about Smyrna, nor about Constantinople, nor in Zante, nor in Corfu, nor about Trieste, nor in Lussin Piccolo, nor in Meleda, nor at Athens, nor in Morea, nor in Ægina, nor in Syra. Therefore we deem it wrong that the error should be constantly transcribed from one geographical work into the other. Certainly a traveller cannot vouch for the impossibility of finding *Chamærops* *humilis* in some secluded spots rarely visited on the eastern shores of the Mediterranean. But even in case that it could be found, the scarcity of so large a plant would require explanation, while the same palm covers, in Barbary and Spain, extensive tracts of country, whose climate and soil are similar to those of the eastern shores of the Mediterranean, where *Chamærops humilis* is wanting. Such glaring errors make us doubtful about other statements of the author—*e. g.*, when he tells us of " oranges ripening in the south of England in the open air as wall-fruit." (P. 354.)

The stereotyped error of *Chamærops humilis* being spread over all the Mediterranean coasts affects some of the most interesting phyto-geographical inquiries which were reported by Ch. Martins in the *Revue de deux Mondes*, as early as 1856, in a paper *sur la Géographie Botanique et ses progrès* :—

" Le palmier nain (*Chamærops humilis*) existe dans le midi du Portugal, dans toute la partie meridionale et occidentale de l'Espagne, il manque dans le Roussillon et le Languedoc, la Corse et le nord de la Sardaigne, mais apparait sur un espace restraint de la côte de Nice et à l'île de Capraia près de Livourne; puis il manque de nouveau dans tout le nord de la péninsule italique: il ne reparait qu'aux environs de la Terracine, sur les limites du royaume de Naples et des états du pape, devient commun dans l'île de Caprée et surtout dans Sicile. Dans la partie orientale de la péninsule italique il se trouve à Tarente, puis en face de la Dalmatie ou il descend jusqu'au golfe de Corinthe, mais il n'existe n en Grèce ni dans les îles de Zante et de Corfou. Trop commun en Algérie, où il est le plus grand obstacle aux défrichemens, on ne le rencontre pas en Egypte mais seulement en Nubie. Aucune consideration géologique ou météorologique n'explique une distribution aussi singulière. Pourquoi le palmier nain manque-t-il dans la Corse et dans la partie septentrionale de la Sardaigne, tandis qu'il se trouve au nord près de Nice, à l'est dans la petite île de Capraia, à l'ouest sur toute la côte d'Espagne? D'anciennes connexions de terres séparées maintenant par la mer peuvent seules rendre compte de cette dispersion capricieuse."

Since October 1856, when this was published, the old phyto-geographical error as to *Chamærops humilis* has been so often repeated even by good authors, that we must try whether the *Philosophical Journal* will eradicate it finally. A plant marking the limits between the region of palms and of laurels is of so much phyto-geographical importance, that erroneous conjectures should yield to true observation, which in this instance is not difficult; for even those who run may read the fact, that phyto-geography is still in its infancy, and that no science would be more advanced than this by a well-organised universal scientific congress, not attempting impossible acclimatisation, but realising most important naturalisations, or at least translocations, like that of the maize and the potato. It belongs to the duties of periodical literature to prevent the spreading of the phyto-geographical errors of great authors—*e. g.*, Barth was mistaken when he wrote about *Borassus flabelliformis* in Central Africa. He saw probably *Borassus Æthiopum.* Livingstone was mistaken when he reported about Cinchona in South Africa. In the magnificent *Historia Palmarum* by Martius, we find the following inaccurate statement about *Chamærops humilis: per maximam partem littorum Maris Mediterranei diffusa offenditur*, while in fact it is scarcely one-fourth of these coasts where this palm occurs. By inaccurate translators has even the limitation *maximam partem* been omitted.

Mr Rudolph commits another mistake, informing us that *Ilex Aquifolium* or holly, growing in more favourable spots of Europe to the size of a tree, occurs in England only as a shrub. The fact is, that we see *Ilex Aquifolium* even at Aberdeen forming trees, and the climate of Britain seems more favourable to holly than that of Germany. It is not to be wondered at that, along with such mistakes committed in Europe, there should be mentioned *Cinchonenwälder auf den Bergen der Tropen*, p. 46 (cinchona forests on tropical mountains). There do not exist any cinchona forests, but only primitive forests, in which, densely surrounded by other trees, some cinchonas are interspersed, which cannot be reared alone after the surrounding thicket of the primæval forest has been destroyed. It would also have been well to tell us what kind of cinchona trees Mr Rudolph mentions as belonging to the flora of *Hawaii*, and on what authority he does so on page 218. He has told us before, on page 202, that *Cinchona condaminea* is confined to a narrow spot in Peru, about the fourth degree of latitude, and to an elevation from 5000 to 7000 feet. He repeats also sometimes the inaccurate term *Chinawälder* (cinchona forests), and seems to be unacquainted with the researches of Weddell, Delondre, Hasskarl, and others, who have proved that there are no such forests, but only cinchona interspersed among other trees of the primæval forest, and have shown the limits in which the cinchona trees grow with much greater accuracy than Mr Rudolph lets us suppose. Botanists, speaking of the genus Cinchona, mean the various species of the trees from which Jesuit's or Peruvian bark is obtained, and not the order of *Cinchonaceæ* to which the *Coffea Arabica* also belongs. Evidently there is in the following statement some error, page 217: *Die mittlere jährliche Wärme von Kanton beträgt* 17° 5′; *die mittlere Sommerwärme* 22° 2′; *die mittlere Winterwärme,* 21° 1′; *and in den Sommermonaten fällt selbst bei Nacht die Temperatur selten unter,* 22° !!* This is quite impossible, because the mean temperature of winter cannot be above the mean of the whole year. Besides, it appears that most of these numbers are too high. Sometimes Mr Rudolph contradicts himself—*i. e.*, he tells us that the *Musa paradisiaca* has the largest leaves in the whole vegetable creation—"*Die gröszten Blätter welche die Natur aufzuweisen hat*," page 108; but subsequently he informs us that the net-palm, *Manicaria saccifera*, in Brazil, has undivided leaves of great tenacity, growing to the enormous size of 20 feet by 6, page 234. Certainly he cannot assert that the very easily lacerated leaves of the Musa ever attained so prodigious dimensions. In fact he himself states their size as 8 or 10 by 2, therefore only one-third of the size above mentioned. Mr Rudolph tells us (page 242), that the doom-palm (*Cucifera thebaica*) has its polar limit at Cairo. In this he has been wrongly informed. The *Cucifera thebaica* has its northern limits some degrees farther to the south than Cairo.

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* The degrees are Reaumur's.

It is also a mistake that every peasant in Egypt cultivates a field full of arrowroot (page 242). Certainly about Cairo and in the whole Delta, the cultivation of this plant is very seldom seen, and almost entirely unknown.

He mentions Syria as belonging to the fatherland of *Phœnix dactylifera* (page 115); but more correctly, he says (page 244), that in the most southern coasts of Palestine, the fruit of the date-palm still ripens, which observation correctly implies, that almost the whole of Syria where the dates do not ripen cannot be the fatherland of the date-palm. In fact, it is seen there rarely, and only as an ornamental tree.

To ten of the cedars on Mount Lebanon, Mr Rudolph ascribes the age of from 3000 to 6000 years, but omits to mention how he arrived at this remarkable fluctuation between about 3000 or 6000 years.

Speaking (page 266) about California, he omits to mention the *Wellingtonia gigantea* and *Taxodium sempervirens*, which certainly belong to the most remarkable botanical phenomena of these regions. Also in the chapter on coniferous trees (pages 30–32), no mention is made of these giants. This omission creates the more surprise when we find the *Wellingtonia gigantea* represented on the adjoined plate. About the *Araucaria imbricata*, we read on page 31 statements which require some amendment. Mr Rudolph says, that this tree, like unto our own coniferæ, casts off its elder branches and leaves, so that its crown is confined to one-fourth of its entire height. Now, this is neither the case with *Araucaria imbricata* nor with our own coniferæ. Neither our *Abies* nor the greater part of our firs cast off their branches. This happens only where trees grow so closely together that the lower branches are suffocated. This may be observed among oaks and beeches as well as among coniferous trees, most of which, where they are not crowded, have a tendency to retain their lower branches, so as to form beautiful obelisks or pyramids rather than crowns. So the *Araucaria imbricata,* where its growth is neither confined by gregarious trees, nor restricted by the gardener's knife, lets the branches of its lowest verticil rest on the ground around the stem, and represents when young, if seen from a distance, a dark-green globe, and later an elongated oval, standing like the egg of Columbus.

According to page 30, the coniferous plants are all distinguished by the straightness and slimness of their stems; but at page 390, we find a picture of *Pinus pumilio* spreading its crooked stem over the ground, and we might add easily another half-dozen exceptions to what Mr Rudolph states to be the characteristic growth of the coniferæ.

Notwithstanding this and other mistakes, the work is a most agreeable manual of phyto-geography. The author has the aim and ability to imitate in his descriptions Humboldt and other masters of style. He has correct views of the importance and the necessary limitations of phyto-geography. If he goes on improving his pages, not merely negatively by avoiding errors, but also positively, by looking at plants such as they are with the precision of systematic botanists, and consulting the writings of Alphonse Decandolle, Weddell, Delondre, Hasskarl, Junghuhn, and other observers, his volume, without losing its attractions for the general reader, will become valuable as a scientific handbook.

On the Origin of Species by Means of Natural Selection, or the Preservation of Favoured Races in the Struggle for Life. By CHARLES DARWIN, M.A. 8vo. London: Murray, 1859.

> "His reason ought to conquer his imagination."—DARWIN.

In the olden time, and in early science, there were many wild and extravagant theories proposed, and their existence lasted for a longer or shorter period, not according to the value of the arguments or facts by which they were supported, but just as the attacks by which they were assailed were numerous and persevered in. In modern days, within the last few years, an anonymous book appeared, the object of which was again to bring before the world of science the origin of the human race and the theory of development. It was so plausibly, so popularly written, and brought forward such an array of what seemed to be facts, apparently gathered from the latest authorities, that by many it was thought dangerous for the general reader, and its principles were challenged and grappled with, discussed and knocked down, by men standing high in science and theology, heads of universities, and by practical workers in the field of nature, who could observe and judge for themselves whether the facts stated were real or not. And thus it was, that instead of the "spirit being laid" it was roused, and the minds of men of various professions, from the army to the church, aye, and of women too, have been stirred up, and they have thought it necessary, for the correction of views judged to be erroneous, to place upon record what they considered the only true version in volumes of the most varied colouring both inside and out. It is true, that previous to the publication of the anonymous book, allegations had been made against geologists for asserting facts which were said to controvert the Mosaic account of creation. The theory by Agassiz of a black as well as a white Adam, published in an American periodical, and republished in this country by the late Professor Jameson, with most of the objectionable passages suppressed,—the appearance of the works of Nott and Gliddon, and the translation of some of the physiophilosophical works of the Germans, all somewhat prepared the way for these books, but the matter was finally clenched by the celebrated battle of the Vestiges, and authors both small and great, are now most numerously developed. Among others, from whom we might have expected better things, "Omphalos" hints at our misunderstanding the language of the sacred record, and makes the origin of all things a very easy matter, by an instantaneously-formed universe, with its various strata, containing ready-made fossils and footprints, glacial scratches and ripple-marks. Agassiz, in his remarkable Essay on "Classification," goes back to the ancient maxim, "Omnia ex ovo;" while the author, the title of whose work we have placed at the head of this article, is convinced that the development theory is the only true solution of the difficulties of the question, and goes boldly into the subject in all its branches, backed by a powerful name and reputation, and supporting himself by a mass of so-called facts. The "Origin of Species" coming before the public under such auspices, and so ably treated as the subject undoubtedly is, we feel constrained not to pass over the work in silence; besides, the time has now gone by when such questions are to be compromised. The more these matters are sifted the sooner we shall reach the truth where attainable at all, and that need not be feared. The facts of geology, where there is an apparent discrepancy, must be explained; and the man of development must have some stronger arguments than he has yet used, to prove that a bear may become a whale, or that an apteryx flew to New Zealand and lost its wings there by disuse.

Mr Darwin states that he has made a large collection of facts, too voluminous for publication in his present work, in which he has only introduced such as bore immediately upon his subject. We regret this much, because we cannot reason upon them nor estimate their value, and it is upon those so-called facts that the whole truth or untruth of his theory rests. Many of his positions are stated hypothetically; some of the statements given as facts are questionable, or are given on the authority of a "careful or good observer," whose careful or good qualities we are not allowed to judge of; and when we read such passages as that we have placed in a note below,* we lose some of the faith we had placed in the great research and learning of the man, and feel almost inclined to doubt the capability of his mind *now* to judge impartially. With such a range and plasticity as Mr Darwin pleads for, we know not where to stop—centaurs, dryads, and hamadryads, and all those remarkable forms we enjoyed so much as schoolboys to read about, but were taught to look upon as mere poetic fancy, may have been really our old progenitors in a transition state to improvement; and when so convinced, with how much pity shall we look down upon the Carsons and Dunbars, Sandfords and Pillans, and blame that ignorance which kept our young minds in such darkness. The "*virginei volucres,*" then supposed to be "peculiar" to the Strophadic Isles, so beautifully figured in the old editions of Virgil; the dangerous siren which is thus described, "desinit in piscem mulier formosa superne," may have all lived; and in latter days, if we follow Darwin, "I can see no difficulty in believing" that mermaids once filled our seas, and that the much disputed sea-serpent exists, or at all events did so within historic time,

> "Prone on the flood, extended long and large,
> Lay floating many a rood."

Notwithstanding our scepticism of Mr Darwin's theory, the "Origin of Species by Means of Natural Selection" is not a book to be trifled with. The author is well known as a man of reputation in science, who has travelled over a great portion of the world, observing as he went; and he does not hide his name, like some ashamed Vestigian, but boldly propounds his theory, and tells us on what it is based. The whole subject, however, is so extensive, that it would require a book as large as Mr Darwin's to go over his ground, or answer his paragraphs seriatim; and we shall only be able to notice its great principle, *Variation* and its laws, and how far these develop, or, as it is termed, improve themselves in a natural state, as it is on this that nearly the whole theory of development under natural selection rests.

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* "In North America the black bear was seen by Hearne swimming for hours, with widely open mouth, thus catching, like a whale, insects in the water. Even in so extreme a case as this, if the supply of insects were constant, and if better adapted competitors did not already exist in the country, I can see no difficulty in a race of bears being rendered, by natural selection, more and more aquatic in their structure and habits, with larger and larger mouths, till a creature was produced as monstrous as a whale." (P. 184.)

Before entering upon this, however, we must explain what "Natural Selection" means. We were for some time at a loss to understand this until we came to Darwin's explanation, "If variations useful to any organic being do occur, assuredly individuals thus characterised will have the best chance of being preserved in the struggle for life; and from the strong principle of inheritance they will tend to produce offspring similarly characterised. This principle of preservation I have called, for the sake of brevity, NATURAL SELECTION." At the beginning of the same chapter, he has added to this, "On the other hand, we may feel sure that any variation in the least degree injurious would be *rigidly destroyed;*" and he includes "sexual selections" as a powerful assistant. The theory is then based upon the animal inclination and capability to vary, and the plasticity of certain groups of animals in a state of domestication is brought forward to show the range to which this may take place.

"*Variation*" in domesticity, by crossing, or under artificial circumstances, is widely different from that in a state of nature. It will not be denied, we presume, that animals were created for the use of mankind. Man was to have dominion over them. In the great group of ruminants among quadrupeds, and rasorial forms among birds, the provision for becoming serviceable to man, breeding in confinement or restraint, and accommodating themselves to circumstances, whether of climate or country, is very marked. These, from the beginning, were made use of; and a few other animals, such as the dog, were, from the very earliest historic periods, chosen to associate with and assist man; and there is no reason to insist that any of those should have a mingled origin; for, if Mr Darwin will apply the same arguments which he has used in the case of the pigeons, all the varieties of which he acknowledges to be descended from one stock (*C. livia*), there does not seem any great difficulty in believing that most, if not all, our domestic animals have also sprung from some one wild animal, although we cannot with certainty now point that one out. The early domesticated animals were cared for and tended, and various points we know were esteemed of more or less value; and, as man became more luxurious and civilised, animals from a distance were introduced and crossed, and the improvement (as it was termed) of cattle and sheep became almost a science; and those breeds and varieties which had a tendency to be most easily fattened, to yield the greatest quantity, or richest milk, or the finest wool, were assiduously cultivated. But this was all artificial. The slightest inattention deteriorated these breeds (that is, returned them nearer to the original form), and those *improved* animals, differing from anything in nature, cannot be *naturally* kept up. Man's species will not last. And here is just the check which God has interposed. These animals are created for the use of man, and his mind has been allowed to exercise itself and study certain conditions under which qualities in the animals are better associated with the wants he has. God in his goodness allows thus far, but he will not permit man to manufacture a *permanent* species, or to sport at will with his works; and this is proved by the fact, that none of those breeds or varieties (Mr Darwin's incipient species) can be maintained, even with the greatest attention and care. Most of our old breeds of domestic cattle do not now exist; some new quality was wanted, the old one was neglected, and the breed *died out.* The same occurred with our breeds of sheep. And where now among dogs is the Turnspit, the Irish blood-hound, the Spanish pointer? They also were not required, and were supplanted. And were the fox or the hare to be extirpated from Great Britain by any cause, the fox-hound and greyhound would immediately follow them.

We think, therefore, that all the arguments brought forward from plasticity in a domestic condition, just prove that it is unnatural and artificial; that the forms desired cannot be kept up, and that it is only when the whole constitution is artificially worked upon, the natural craving for food kept blinded by constant supply, the natural passion of the sexes curbed by the allowance of some particular *improved* form only being admitted, with which the beast must either satisfy his natural inclination or want.

The same principles prevail in plants. It is by the art of the gardener or agriculturist that we have our melting pears and incomparable dahlias, with almost all our useful varieties of garden and field vegetables; but can we carry on or maintain these by a natural process? They stand in a stronger position than even the forced varieties of animals. They cannot be produced by impregnation and seeds; all our finest fruits must be layered, budded, or grafted; and even such plants, according to the theory of Mr Knight, which has not been disproved, die with the parent stocks. Where, now, are many of our old much-prized varieties, such as the golden pippin? &c. Florists' flowers are mostly propagated by cuttings; no seed is certain to produce the "incipient species" it was sown from. Our kitchen vegetables—cabbage, cauliflower, broccoli, Brussels sprouts, Savoys, curled greens, &c., all, like the pigeons, spring from one stock, and are kept up only by the greatest care of the nurseryman, whose profit it is to do so. And how often have we to complain of "bad seed," that is, seed returning offspring nearer to its normal state. The same is the position of our agricultural grains, turnips, &c.; they are all changed and changing. Skirving cannot keep up his Swedish turnip; he is obliged to bring out new varieties; and the same causes that have changed our breeds of cattle are acting on our breeds of vegetables. And what will become of these manufactured species; their constitutions placed in circumstances foreign to them, and excited by manures and stimulating mixtures, have been weakened, and from the vine to the potato they have died out, or become so precarious in their produce as in many instances to be given up.

It is argued, that such artificial breeds of animals would never go back to their original form; very likely not; they never would have the opportunity. The countries where they roamed are now cultivated and peopled—their ancient food and quiet destroyed; they would have no real species to mate with, and the limited variety would breed in and in as long as it could, and at last dwindle and die. But under favourable circumstances many would go back. In a lately published account of New Zealand, it is mentioned that the pigs introduced there by Captain Cook have been naturalised, and " in the deep recesses of the forests they have lost the appearance of domestic pigs, and have acquired the habits and colour of wild animals." Among plants, the reversion to the original form is common and constant, and takes place within a very short period; witness the camellia, dahlia, rose, daisy, &c. &c., and most of our cultivated garden vegetables.

In a natural condition, *variation* by crossing is very different. Mr Darwin has drawn most of his arguments from birds. We shall take these also. As a general rule or law, birds do *not mingle* or interbreed; even allied species frequenting the same localities do not. Mr Darwin has given us no proof that they do, or ever did. Those instances given by authors are all traceable to circumstances occurring at variance with the usual habits—such as that of a winter migratory bird being detained from some cause and mating with an allied species in spring; but even in this case, what would it lead to? The cross would, it is acknowledged, not breed among themselves, and with a true bird of either side it would soon be lost. The pheasant has always been quoted as an instance of two species breeding in a wild state; it is just a proof of the reverse. We have not a true pheasant in all Great Britain. The ring-neck species of China, and the dark coloured species of Persia, have been both introduced. They have been crossed and recrossed, are altogether in an artificial state—are fed, bred in confinement, and sheltered, and so they go on under care; but it is well known to every sportsman that they can only be kept up by the introduction of " new blood," as it is called; and without this, "preserves" decrease, birds will not breed, and the stock is, or would be gradually lost. But in other instances of allied species said to interbreed, such as *Passer hispanoliensis* and *cisalpinà*, *Corvus dauricus* and *monedula*, &c., nothing, we maintain, very different would arise. The sparrows might breed together, they and their families for ever, they never would produce a bunting or a bullfinch, or the two jackdaws a pigeon, or even the white-necked crow of Africa.

In all those nearly allied species inhabiting different countries, Europe and America for instance, the minute separating characters are *always the same.* The ducks have a great power of flight and are widely distributed, and have every opportunity of intermingling with birds from a distance or closely allied. No difference or intermediation can be detected between the common wild ducks (*Anas boschas*) of Europe, North America, or India. They "sport" widely in domesticity, but, when unrestrained, never vary, no matter how different the countries, climates, and food may be. The American teal again, which an ordinary observer would scarcely detect among a series of European birds, has a white band on each side the breast, and other marks. The American goosander varies still less but is also easily distinguished; and so also the velvet scoter, and some others; but the distinguishing marks in these birds are always the same — quite constant. We do not yet know the geographical range of these, and other birds, sufficiently to say where allied species meet; and we never see intermediate forms now. If at their meeting they were not prevented by natural instinct from intermating, intermediate forms would be produced, and would be circulated, which, so far as we know from very extensive observation, is not the case. In all animals, however, there is a certain natural range of variation, amounting in some to almost nothing, in others very wide; you may place a hundred goshawks, wild ducks, European teal, and American teal, European goosanders, and American goosanders together, and you will scarcely find a feather different in each species; but you may take the same number of specimens of the common European buzzard, or of the ruff in its breeding plumage, and you will not find two similar. We are not, however, entitled to conclude from this last fact that either the buzzard or the ruff sprung from anything very different from what they now are, or that they would ever be developed into anything very different.

Mr Darwin has insisted largely on the influence that interbreeding has on variation and development, which we have endeavoured to show has been much overrated. But it is by "natural selection" that his development system is to be mainly carried out; and this, he says, is a power "immeasurably superior to man's feeble efforts," and should therefore produce far more remarkable modifications and changes than can ever be accomplished by man's scientific breeding, and turn the incipient variety at last to a permanent species, widely differing in structure and form from its ancient progenitor." The strongest and most improved will be "rigidly preserved." Now, how does this work among living species, as we term them? The strongest and most powerful buck will drive off the younger and weaker, and remain master of his herd. The strongest black-cock will drive off all intruders from his particular green hillock. But will either of these in consequence beget the finest progeny? Will it be the strongest and healthiest females only that attend upon their polygamic lords? Weak and even diseased females may fancy a well-formed stately mate. Overwork may weaken the powers of the most powerful male, and the beaten herd would produce the healthier stock. Many of the prize animals at our late agricultural shows have been gotten by *young* males. But allowing, for the sake of argument, that there may be improvement by natural selection, that is, greater proportional size and strength, and with all the additions that shelter and quiet, and abundance of food and water could give, could the progeny of the fallow or red deer, under any circumstances, ever be developed into a moose, or elk, or wapiti, or the wild-cat of Europe into a tiger or lion. Yet such, according to Mr Darwin, must be the case, "as I believe that all the species of the same genus have descended from a single parent," Can such be possible under the laws which scientific men have been accustomed to think regulated animal life? Have we any like analogy in all the range of animal and vegetable organisation to speculate upon? can we, with all our knowledge, reach the process by which these transformations are to be accomplished? We simply answer, no; but Mr Darwin meets us by saying, that it was not from a fallow-deer, or stag, or wild-cat, that these larger forms were developed, but from some "ancient prototype of which we know nothing," never saw, and till now never heard of—quite unlike anything now in existence—and that the time required to bring about the monstrous change would be "millions of millions of ages." And he adds, in his concluding chapter, That "as all forms of life are the lineal descendants of those which lived long before the Silurian epoch, we may feel certain that the ordinary succession by generation has never been once broken, and that no cataclysm has desolated the whole world. Hence we may look forward with some confidence to a secure future of equally inappreciable length. And as natural selection works solely by and for the good of each being, all corporeal and mental endowments will tend to progress towards perfection." These are the deliberate, serious assertions of the author. We can do nothing with them. There never will, nor can be, except in imagination, any means of tracing back millions of millions of ages. All that we now know fixes the immutability of once-created species. In the Tertiary strata many of the mollusca are exactly similar to those now inhabiting our seas and fresh waters; and in our limited historic time, animal forms are depicted on monuments above three thousand years old, just as they now are; and the sacred Ibis embalmed in the ancient catacombs does not vary in a bone or feather from that now feeding by the Nile. All that we have quoted above are mere assumptions, not the calm reasonings of a man of science. Surely Mr Darwin has also for a time forgotten the igneous rocks of his geology, and overlooked the Bible statement, "the world that then was being overflowed with water perished," and that "the elements shall melt with fervent heat, the earth also and the works that are therein shall be burned up."

But it is not always to development that Mr Darwin restricts himself. "Disuse" is a great agent in the change of form:—thus the progenitor of a seal "had not a flipper, but a foot with five toes fitted for walking and grasping." The same may have been the case with the walrus, and the curious whale-like pachyderm of the lakes of Central Africa. "I believe," he writes, "that the wingless condition of several birds which now inhabit, or have lately inhabited, several oceanic islands tenanted by no beast of prey, has been caused by disuse," and the eyes of the mole are a "gradual reduction from disuse." All these circumstances, however, if true, would act against Mr Darwin's theory of "*improvement*," which he states would always be "rigorously preserved." We should call these retrogression from perfection of structure; but it is little matter, as we think them as untenable as improved development.

The theory of some old progenitor of nameless shape, differing from anything now in existence, is to us an unintelligible brain-myth; it is the assumption of a monstrous fact, not a reality—a thing whereon we have no premises to argue upon; and the process of development from that, or, even later, from any form actually before us, trenches so far upon all the attributes of foreknowledge and design which we have of old given to the Creator, that we would ask the beginners, and students of natural science, before many of whom this book will undoubtedly come, to view the theory with the greatest distrust, and to sift every fact and every argument in it to its very bottom. We do not think Mr Darwin intends to lower the power or attributes of Deity—we would gather the reverse from many of his expressions; but when he writes of "mere chance" causing one variety to differ from its parents—when he likens the electric organs present in fishes (for which he can find no ancient progenitor)—so rare, and placed in such different forms—just like separate inventions of two men differently hit on; when he calls creation "a Theory," and speaks of working "in a tail for all sorts of purposes," "a new variety raised by men will be a far more important and interesting object for study than one more species added to the infinitude of already recorded species"—to say the least, it is not the language in which we have been accustomed to see these subjects treated. Wingless birds, remarkable only because we are wont to consider a wing inseparable from a bird, filling their allotted position, and in every way provisioned for it, whether on land or water; blind animals filling the caves and "waters under the earth;" the birds of the waters so beautifully arranged in all their internal and external appliances for the lives they lead; the birds of the air, the swallow, or the owl, so finely organised for their purposes—long migration, or a noiseless, "downy" flight or nightly vision; the seal, so wonderfully adapted to its watery life; the mole, in all its structure; these and a thousand thousand more due only to creative power and knowledge, are, according to Mr Darwin, the proceeds of mere development, disuse, disease. Can this be so?

Through the whole of Mr Darwin's most interesting volume we find such passages as these—"The result of the various, quite unknown, or dimly seen laws of variation is infinitely complex and diversified." "The laws governing inheritance are quite unknown." "Our ignorance of the laws of variation is profound." "We are profoundly ignorant of the causes producing slight and unimportant variations." "Variability is governed by many complex laws," &c. &c. Are we then yet in a position to judge, or can Mr Darwin settle the question so confidently as he does, under such admissions of ignorance as we have now quoted? We think we are not yet prepared to do so. We have said at the commencement of our remarks that some of Mr Darwin's facts were questionable, and it is perhaps not fair to do this without stating some, though it is impossible here to go into any separate discussion of them. Many of the facts are second-hand, and without authority given. Of others, we dispute that disease was or is present in the eye of the mole: we dispute the La Plata woodpecker never climbing trees, or the upland goose not swimming; we dispute the restricted condition of the Galopagos Fauna and Flora; we dispute Horner's calculations of the Nile Delta; and we dispute several geological positions, &c. &c. All the facts require sifting and analysis, and that is now the duty of our young and active working zoologists; we would say it is also the duty of our clergymen, for the questions will come before them whether they will or not. Let them take them up boldly and without reserve. Let them sift every old fact, and search out as many new ones as they can, we have no fear for the result; but let them do this impartially and with the view to the truth only, and if it is discovered, we shall owe a very large debt to Mr Darwin for having given us so good a scent. We say hunt the OLD PROGENITOR out, and run into him breast high.

PROCEEDINGS OF SOCIETIES.

British Association for the Advancement of Science. Meeting at Aberdeen, September 1859.

(Continued from page 141).

ZOOLOGY AND BOTANY INCLUDING PHYSIOLOGY.

The **President**, in opening the Section, said,—In the early days of the British Association, the circumstances that were considered desirable—nay, in some instances that were thought indispensable to insure a successful meeting, were—a populous neighbourhood, connected in some way with learning, or with commerce or manufacture, and to which there was an easy and rapid access. Thus the capitals, English universities, and large towns, were our early choice. In 1833–34 or –35, a meeting in Aberdeen would have been judged to have been nearly impossible; and the inhabitants of this great city may now look back with satisfaction upon their own enterprise. For, by the application of the principles of steam, and mechanical and engineering science, distance, and time, and expense have been so much reduced, that thousands of persons are enabled to meet together from all parts of the world, and to commune with each other over the mighty agents which God has placed within their power. To the same causes we are indebted for the high position this great body now holds. Opportunity is afforded to many of those whose time is otherwise necessarily employed, to assist, by their countenance and presence, the discussion of subjects on which depends the high place our country has taken among nations. And thus it is that the peace of the world will be best preserved. For while on the one hand mind and science are engaged in contriving and perfecting engines of destruction, possessing power and range far beyond what was ever conceived possible, the means of intercommunion between the various nations of the world at the same time are daily widening and expanding. I think, then, under these circumstances, I am entitled to offer my congratulations upon our meeting here, and to express the satisfaction that I feel in again joining the many old friends and associates that I see around. And if a graver feeling sometimes steals over, at the absence of those whom we were wont to meet, that is softened and brightened by the sight of many new faces, that have, I trust, come to assist and take part in our discussions.

Since we met last year in Leeds, Zoology and Botany have steadily advanced. In Great Britain and Ireland, of the works which have been commenced in former years some have been completed, and others go on with their wonted energy. The fine works incident to the Government Expedition brought out at the public expense, and under charge of the Lords Commissioners of the Admiralty, have been mostly completed, with one exception, to which, we trust, the attention of Government will be directed by some of our scientific friends in Parliament: it is the Zoology of the Expedition of the Erebus and Terror, from 1839 to 1843. This was commenced in 1844, and, after a period of fifteen years, yet remains unfinished. The contributions to the natural history of Labuan, and the adjacent coasts of Borneo, by Mr Motley and Mr Dillwyn, so beautifully commenced a few years since, and illustrating a Fauna little known, have not been continued, and will, I much regret, cease to be so under their original authors; for, in the fearful massacre that took place at Kalangan on the 1st of May last, Mr Motley and his family were the first to fall victims to the rage of the natives. This unhappy loss will be a serious one for Science. Mr Motley laboured hard in our particular walks; and being chief engineer of the coal-mines in the eastern division of Borneo, he also turned his mind to Geology—and at the time of his death was preparing a paper for this very Meeting upon the coal of those countries, and upon "The Progress and Growth of New Coal Formations now preparing for Future Ages." It may be recollected that among the grants of money appropriated at our last Meeting to Section D, there was one given to assist Mr Eyton in his work intended to illustrate the comparative osteology of birds. Two beautiful numbers of the work have already appeared, and the third is ready for publication. The periodicals devoted to zoology and botany continue to be well conducted. In these and in the Transactions of Learned Societies much facility and encouragement is given to the publication of valuable Memoirs; and I may mention that in one branch (Ornithology) which has not yet maintained a periodical for itself, an experiment is being tried in Mr Sclater's "Ibis," of which the first year's numbers will be completed in October. The importance of Publishing Societies has been generally acknowledged. Many of us are members of the *Ray Society*, devoted to furthering the objects of our Section, and it gives me pleasure to lay before you Professor Huxley's beautiful volume on "Oceanic Hydrozoa," observed during the voyage of H.M.S. Rattlesnake, now ready for subscribers; and also the drawings and plates of Mr Blackwall's volume on Spiders, now far advanced. The members of our Learned Societies have occasionally founded medals or prizes for the encouragement of men of science. You will see presented to Sir R. Murchison during this Meeting the medal founded by Sir T. Brisbane, President of the Royal Society of Edinburgh. The late Dr Patrick Neill founded another medal, which has been this year awarded to a botanical work of rare excellence and beautifully illustrated, "The Reproductive Organs of Lichens," by Dr L. Lindsay.* In Ireland, the Rev. C. O'Meara's works on "The Reproduction of the Diatomaceæ" hold a first place. Mr Archer's papers "On the Desmidieæ" are also able. In Zoology, marine life has been most advanced by Dr Kinahan, Professors Green and W. King; while in the Dublin University a lectureship in zoology has been founded, and shows its value by being well attended. The condition of our public museums is a very important subject. Their condition is becoming more healthy. The discussions upon the accommodation in our noble national collections, and of the propriety of the separation of the Literary and Art Departments from the Physical, will, I have no doubt, bring out results favourable to both. One great and important feature is the arrangement and cataloguing of our public collections. The officers of the British Museum have worked hard in these departments, and its Catalogues now reach to a numerous and valuable series of volumes. Some of these are well illustrated, while others are almost monographs. This year Dr Gray has devoted one to a portion of the Batrachians or Frogs, and Mr F. Smith has published an excellent part "On the Fossorial Hymenoptera." The University Museum of Edinburgh is one of great value, as, besides possessing the rich mineralogical collection made by its late able Professor Jameson, it gained by purchase the entire zoological collection of the late M. Dufresne of Paris, in which are many of the type-specimens mentioned and described in the zoological works published at the end of the last and beginning of the present century. The formation of a Museum of Technology under Professor George Wilson will, I trust, improve the condition of this part of the University; but at present the accommodation and income allowed for museum purposes are not nearly sufficient, and it is impossible for the Regius Keeper to catalogue or arrange, or even preserve, the collection, or to give that aid to study required at the present time, without considerable additions to his staff of assistants. Among the more local collections, the East India House has set a fine example by publishing two excellent volumes, prepared by their late venerable curator, Dr Horsfield. In his task of preparing this catalogue he has been assisted by Mr F. Moore, his under-curator. The Derby Museum of Liverpool will soon, we may hope, follow the same course; it is a most valuable one, and contains many unique specimens from our early expeditions. Its curator is quite able for the task. The Museum of the University of this city has, I am glad to say, been much improved, and a local collection far advanced. I may remark that museums of this class should not, as is too often the case, attempt a general collection. The great object should be to obtain *typical specimens*, so as to illustrate and explain the subjects and the geographical distribution of animal life in particular forms; afterwards, a good British collection should be brought together; and, lastly, the local Fauna and Flora should be illustrated. Aberdeenshire, from its seabord and a country stretching inward to a great elevation, is very rich, and the native animals and plants are becoming extirpated and "forgotten." Another object should be the illustration of any branch of industry or commerce, for which there is a wide field here in the Arctic fisheries. But the one great character of the present time is that of popular information—popular works on all subjects. This is no doubt all in the right direction, and shows the call for information; but it may be overdone. False information is worse than none. Some of our great principles cannot be studied against time, and diluted chapters from authors of reputation sometimes neither give the truth nor the author's meaning. These form a considerable staple in our weekly press. It is your duty, then, who are presumed to know something of the various branches you profess, to inform, and counsel, and advise, as far as you can, the authors of those lesser works, when they will take advice, and to endeavour that at least accuracy is carried out in their endeavours to instruct others.

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* Now published in volume xxii. of Transactions of the Royal Society of Edinburgh.

Upon the continent of Europe the progress of Zoology and Botany has been steady. In our Foreign possessions there is an advance. The melancholy events that have occurred in India, and her unfortunate position, have given a temporary check there; yet the scientific journals of that country, which have brought so much to light continue, and there is no country where we have been so much indebted to our military officers for physical information. Their names would form a very long list. Colonel Sykes, your member of parliament, now here, deserves every praise; and among Scotchmen you have Elliot and Jerdan, M'Clelland and Adams, the latter an Aberdeenshire man, and who has brought many new objects of interest to this country. But it is in the younger countries where we see an advance more evident. Australia and Van Diemen's Land, now that wealth permits time and luxury, have attended to science. In most of the journals of these countries we have original observers, and by and by we shall have the results of the study of the remarkable productions of these lands made where they live and grow. New Zealand also has its scientific journal. It is, however, in the New World where the greatest activity at present prevails. She has already, with credit to herself, sent out scientific expeditions of a general character, and those of Wilkes and Rae and Kane are well known, and huge works have sprung from each; the vast extent of territory now claimed by the

American people has given rise to surveys and exploratory expeditions at home, and these are proceeding in all directions to fix the boundary-lines and the best railway routes to the Pacific,—naturalists and draftsmen, in fact all the necessary staff, accompanying each expedition,—the results of which are published in reports to Congress, in which they are assisted by the Smithsonian Institution of Washington. But the work of the greatest magnitude and importance to America is "Contributions to the Natural History of the United States," by Agassiz, originally advertised to be completed in ten large volumes, but the subscription has been so great as to allow the extension of the work beyond the contemplated limits. Two volumes for the first year, on the Testudinata or Tortoises, have been published, illustrated by thirty-four plates. An important part of these volumes is an introductory essay, which has been republished separately in an 8vo volume. Louis Agassiz's "Essay on Classification," embraces the whole range of the subject, which he treats in a wider and more comprehensible and less mechanical manner than has hitherto been done; but while I thus praise the work and the manner in which it is treated, and agree with a great many of the positions he has taken up, I must warn its readers that some subjects are treated in a way Professor Agassiz will not be able to maintain, and that to those who are unable or unwilling to think for themselves, the author's reputation will prove a guarantee not altogether to be trusted. It must be studied with great care and great caution. Nevertheless I look upon it as the remarkable book of the year. There is another work upon a similar subject advertised, from which we may expect some curious reasonings, "On the Origin of Species and Varieties," by Charles Darwin.*

Let me now say a word for Section D. At the first meeting in York, in 1831, the Committee of Sections was naturally small. Zoology and Botany did not come forward in great numbers, and we had only five members, Daubeny, Greville, Henslow, Lindley, and Dr Pritchard. There was no Botanical paper, and only one on Zoology, "On the Crystalline Lens of Vertebrata," by Dr, now Sir David, Brewster. In 1832 and 1833 the British Association met in Oxford and Cambridge—in 1834 at Edinburgh, where the attendance was greater than on any previous occasion, 1298 tickets being issued—Dublin in 1835. These first four meetings are extremely interesting, and a perusal of the volumes containing the Reports will show you how this now great body thought and acted in its early days; how it has crept on, and increased and matured its plans, until it reached the high position in science which it now holds; and that I may not be said to speak too highly of ourselves, or to state matters for which there is no foundation, the work of Section D, since the 27th of September 1831, up to the conclusion of the Meeting for 1858, gives the following results:—There have been read—Reports, 95; Papers, Zoological, 411; Botanical, 213; or, in all, 719 Reports and Papers; and the amount of money granted to Section D for scientific encouragement during the same period appears to have been about L.1007. After the position that I have mentioned to you that the literature of our subject holds, I do not think that we can complain either of slowness or want of interest. Perhaps we have not been so popular as the members of Section C, but we shall not quarrel about which is the more important, as I think we are mutually dependent on each other, and cannot well go on separately. Their science allows great scope for the imagination, and that may occasionally run riot. They have in charge the two great materials of which we all acknowledge the importance, and without the assistance of which we would not now be assembled here—coal and iron. We deal more in facts; but if our members would only look around them, they would soon perceive that nearly all their necessaries and luxuries, whether of food or clothing, or of the adornment of their mansions, are furnished by animal and vegetable products. This, however, depends upon ourselves, and if we will study these wonderful productions with minds impressed with the power and goodness of God in placing them around us, we shall find the investigation of them no weary work, but one full of interest and information. By these remarks I do not wish to claim for the British Association any undeserved influence; but it is now universally acknowledged that the example it has shown, and the various links it has joined between the different departments and the people cultivating them, have had a very decided influence on the promotion of science. At all the meetings of this Association which I have attended I have observed a great impulse given both in the preparation for the meetings and after their conclusion, and if you will give it your attention, you will find that after we have left you, various matters will appear in other lights than you formerly viewed them. Various subjects will be suggested to you, and many of you will try to study and master this or that subject as your inclination leads, and my wish is that you may *persevere* and be *successful.*

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* "On the Origin of Species by Means of Natural Selection," &c., now published. See notice at p. 280 of the present No. of this Journal.

On the Orders of Fossil and Recent Reptilia and their Distribution in Time. By Professor Owen.—Professor Owen began by remarking that, with the exception of geology, no collateral science had profited so largely from the study of organic remains as zoology. The catalogues of animal species had received immense accessions from the determination of the nature and affinities of those which had become extinct, and much deeper and clearer insight had been gained into the natural arrangement and subdivision of the classes of animals since palæontology had expanded our survey of them. Of this the class Reptilia, or cold-blooded air-breathing Vertebrates, afforded a striking example. In the latest edition of the "Règne Animal," of Cuvier, 1829, as in the "Elémens de Zoologie" of M. Edwards, 1834–37, and the still more recent monograph on American Testudinata by Agassiz, 4to, 1857, the quadruple division of the class, proposed by Brongniart in 1802, was adhered to,—viz., Chelonia (tortoises, turtles), Sauria (crocodiles, lizards), Ophidia (serpents), Batrachia (frogs, newts); only the last group is made a distinct class by the distinguished Professor of the United States:—"After this separation of the Batrachians from the true Reptiles, we have only three orders left in the class of Reptiles proper,—the Ophidians, the Saurians, and the Chelonians" (l. c. p. 239). In Professor Owen's Reports on British Fossil Reptiles to the British Association in 1839 and 1841, it was proposed to divide the class into eight orders, viz.,—Enaliosauria, Crocodilia, Dinosauria, Lacertilia, Pterosauria, Chelonia, Ophidia and Batrachia, which were severally characterised. Subsequent researches had brought to light additional forms and structural modifications of cold-blooded air-breathing animals now extinct, which had suggested corresponding modifications of their distribution into ordinal groups. Another result of such deeper insight into the forms that have passed away has been the clearer recognition of the artificiality of the boundary between the classes Pisces and Reptilia of modern zoological systems. The conformity of pattern in the arrangement of the bones of the outwardly well-ossified skull in certain fishes with well-developed lung-like air-bladders (Polypterus, Lepidosteus, Sturio), and in the extinct reptiles, Archegosaurus and Labyrinthodon; the persistence of the notochord (*Chorda dorsalis*) in Archegosarus as in Sturio; the persistence of the notochord and branchial arches in Archegosorus and Lepidosiren; the absence of occipital condyle or condyles in Archegosarus as in Lepidosiren; the presence of teeth with the labyrinthic interblending of dental tissues in Dendrodus, Lepidosteus and Archegosarus, as in Labyrinthodon; the large median and lateral throat-plates in Archegosarus as in Megalichthys, and in the modern fishes Arapaima and Lepidosteus;—all these characters, as the author had urged in his Lectures at the Government School of Mines (March 1858), pointed to one great natural group, remarkable for the extensive gradations of development, linking and blending together fishes and reptiles within the limits of such group. The salamandroid (or so-called "sauroid") Ganoids—Lepidosteus and Polypterus—are the most ichthyoid, the Labyrinthodonts the most sauroid, of the great group; the Lepidosiren and Archegosaurus are intermediate gradations, one having more piscine, the other more of the reptilian character. Archegosaurus conducts the march of development from the fish proper to the labyrinthodont type; Lepidosiren conducts it to the perennibranchiate, or modern batrachian, type. Both forms expose the artificiality of the ordinary class-distinction between Pisces and Reptilia, and illustrate the naturality of the cold-blooded Vertebrates, or "Hæmatocrya" ἁιμα, blood, κρυος, frost: (the correlative group is the "hæmatotherma"). Reptiles are defined as "cold-blooded, air-breathing Vertebrates"; but the Siren and Proteus chiefly breathe by gills, as did most probably the Archegosarus. The modern naked Batrachia annually mature, at once, a large number of small ova. The embryo is developed with but a small allantoid appendage, and is hatched with external gills. These are retained throughout life by a few species; the rest undergo a more or less degree of metamorphosis. Other existing reptiles have comparatively few and large eggs; and the embryo is enclosed in a free amnios, and is more or less enveloped by a large allantois. It undergoes no marked transformation after being hatched. On this difference the Batrachia have been by some naturalists separated as a distinct class from the Reptilia. But the number of ova simultaneously developed in the viviparous land salamanders is much less than in the siren, and not more than in the turtle; and, save in respect of the external gills, which disappear before or soon after birth, the salamander does not undergo a more marked transformation, after being hatched, than does the turtle or crocodile.* It depends, therefore, upon the value assigned to the different proportions of the allantois in the embryo of the salamander and lizard whether they be pronounced to belong or not to distinct classes of animals. This embryonic, or developmental character, is unascertainable in the extinct Archegosaurus and Labyrinthodon. The affinity of Labyrinthodon to Ichthyosaurus, and those structures which have led the ablest German palæontologists to pronounce the Labyrinthodonts to be true Saurians, under the names of Mastodonsaurus, Trematosaurus, Capitosaurus, &c., may well support the conjecture that modifications more "reptilian" than those in Salamandra may have attended the development of their young. Characters derived from the nature of the cutaneous coverings equally fail to determine the class characters of Batrachia as contra-distinguished from Reptilia. It is true that all existing Batrachia have a scaleless skin, or very minute scales (Cæcilia), but not all existing reptiles have horny scales. The crocodiles and certain lizards show a development of dermal bones similar to that in certain placoid and ganoid fishes. This development is greater, and the resemblance is closer, in those ancient forms of Reptilia which exhibit in their endo-skeleton unmistakeable signs of their affinity to ganoid fishes and Batrachia. In a survey, therefore, of the present known forms of cold-blooded, air-breathing Vertebrates, recent and fossil, Professor Owen could not define any real and adequate boundary for dividing them primarily into two distinct classes of Batrachians and Reptiles. As little was he able to point out a character dividing the air-breathing from the water-breathing Hæmatocrya—the reptiles from the fishes. In the present communication the author drew an arbitrary line between Lepidosiren and Archegosaurus, and proposed to begin his review of the ordinal groups of Reptilia, or air-breathing Hæmatocrya, with that of which the Archegosaurus was the type.

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* The Cæcilia may probably depart still further from the type-batrachian mode of development, and approach more to the type-reptilian mode.

Order I. GANOCEPHALA.—For this group or order he proposed the name of Ganocephala (γανος, lustre, κεφαλη, head), in reference to the sculptured and externally-polished or ganoid bony plates with which the entire head was defended. These plates include the "*postorbital*" and "*super-temporal*" ones, which roof over the temporal fossæ. No occipital condyles. The teeth have converging inflected folds of cement at their basal half The notochord is persistent; the vertebral arches and peripheral elements are ossified; the pleurapophyses are short and straight; pectoral and pelvic limbs, which are natatory and very small; large median and lateral "throat-plates;" scales small, carinate, sub-ganoid; traces of branchial arches. The above combination of characters gives the value of an ordinal group in the cold-blooded Vertebrata. The extinct animals which manifest it were first indicated by certain fossils discovered in the sphærosideritic clay-slate forming the upper member of the Bavarian coal-measures, and also in splitting spheroidal concretions from the coal-field of Saarsbruck near Treves; these fossils were originally referred to the class of fishes (*Pygopterus lucius*, Agassiz). But a specimen from the "Brandschiefer" of Münster-Appel presented characters which were recognised by Dr Gergens to be those of a Salamandroid reptile.* Dr Gergens placed his supposed "Salamander" in the hands of M. Hermann von Meyer for description; who communicated the result of his examination in a later number of the under-cited journal.† In this notice the author states that the Salamander affinities of the fosil in question, for which he proposes the name of *Apaeton pedestris*, "are by no means demonstrated."‡ "Its head might be that of a fish, as well as of a lizard, or of a batrachian." "There is no trace of bones of limbs." M. von Meyer concludes by stating that, "in order to test the hypothesis of the Apateon being a fossil fish, he has sent to Agassiz a drawing, with a description of it." Three years latter, better preserved and more instructive specimens of the problematical fossil were obtained by Professor Von Dechen from the Bavarian coal-fields, and were submitted to the examination of Professor Goldfuss, of Bonn; he published a quarto memoir on them, with good figures, referring them to a Saurian genus, which he calls Archegosaurus, or "primeval lizard,"—deeming it to be a transitional type between the fish-like Batrachia and the lizards and crocodiles.§ The estimable author, on the occasion of publishing the above memoir, transmited to Professor Owen excellent casts of the originals therein described and figured. These casts were presented by the Professor to the Museum of the Royal

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* "Mainz, Oktober, 1843.—In dem Brandschiefer von Münster-Appel. in Rhein-Baiern, habe ich in vorigen Jahre einen Salamander aufgefunden. Gehört dieser Schiefer der Kohlen-formation? in diesene falbe wäre der Rund auch in anderen Hinsicht interessant." Leonhard und Bronn, Neues Jahrbuch für Mineralogie &c., 1844, p. 49.

† Ibid. 1844, p. 336.

‡ "Ob das—Apateon pedestris—ein Salamander-artiges Geschöpf war, its keineswegs ausgemacht."

§ "Archegosaurus: Fossile Saurier aus dem Steinkohllengebirge die den Uebergang des Ichthyoden zu den Lacerten und Krokodilen bilden." p. 3. "Beitrage zur vorweltlichen Fauna des Steinkohlengebirges." 4to. 1847.

College of Surgeons, London, and were described by him in his "Catalogue of the Fossil Reptiles," in that Museum (4to. 1854). The conclusions which Professor Owen formed thereupon as to the position and affinities of the Archegosaurus in the reptilian class, are published in that Catalogue, and were communicated to and discussed at the Geological Society of London (see the "Quarterly Journal of the Geological Society," vol. iv. 1848). One of the specimens appeared to present evidence of persistent branchial arches. The osseous structure of the skull, especially of the orbits, through the completed zygomatic arches, indicated an affinity to the Labyrinthodonts; but the vertebræ and numerous very short ribs, with the indications of stunted swimming limbs, impressed the writer with the conviction of the near alliance of the Archegosaurus with the Proteus and other perennibranchiate reptiles. This conclusion of the affinity of Archegosaurus to existing types of the reptilian class is confirmed by the subsequently discovered specimens described and figured by M. von Meyer, in his "Palæontographica" (Bd. vi., 2te Hef. 1857), more especially by his discovery of the embryonal condition of the vertebral column*—*i.e.*, of the persistence of the notochord, and the restriction of ossification to the arches and peripheral vertebral elements. In this structure the old carboniferous reptile resembled the existing Lepidosiren, and afforded further ground for regarding that remarkable existing animal as one which obliterates the line of demarcation between the fishes and the reptiles. Coincident with this non-ossified state of the basis of the vertebrate bodies of the trunk is the absence of the ossified occipital condyles, which condyles characterise the skull in better developed Batrachia. The fore-part of the notochord has extended into the basisphenoid region, and its capsule has connected it, by ligament, to the broad, flat ossifications of expansions of the same capsule, forming the basi-occipital or basi-sphenoid plate. The vertebræ of the trunk in the fully developed full-sized animal present the following stages of ossification. The neurapophyses coalesce at the top to form the arch, from the summit of which was developed a compressed, subquadrate, moderately high spine, with the truncate, or slightly convex, summit, expanded in the fore-and-aft direction, so as to touch the contiguous spines in the back: the spines are distinct in the tail. The sides of the base of the neural arch are thickened and extended outwards into diapophyses, having a convex articular surface for the attachment of the rib; the fore-part is slightly produced at each angle into a zygapophysis looking upward and a little forward; the hinder-part was much produced backwards, supporting two-thirds of the neural spine, and each angle developed into a zygapophysis, with a surface of opposite aspects to the anterior one. In the capsule of the notochord three bony plates were developed, one on the ventral surface, and one on each side, at or near the back part of the diapophysis. These bony plates may be termed "cortical parts" of the centrum, in the same sense in which that term is applied to the element which is called "body of the atlas" in Man and Mammalia, and "sub-vertebral wedge-bone" at the fore-part of the neck in Enaliosauria. As such ventral or inferior cortical elements co-exist with seemingly complete centrums in the Ichthyosaurus, thus affording ground for deeming them essentially distinct from a true centrum, the term "hypapophyses" had been proposed by Professor Owen for such independent inferior ossifications in and from the notochordal capsule, and by this term may be signified the sub-notochordal plates in Archegosaurus, which co-exist with proper "hæmapophyses," in the tail. In the trunk they are flat, subquadrate, oblong bodies, with the angles rounded off; in the tail they bend upwards by the extension of the ossification from the under to the side-parts of the notochordal capsule—sometimes touching the lateral cortical plates. These serve to strengthen the notochord, and support the intervertebral nerve in its outward passage. The ribs are short, almost straight, expanded and flattened at the ends, round and slender at the middle. They are developed throughout the trunk and along part of the tail, co-existing there with the hæmal arches, as in the menopome.[*] The hæmal arches, which are at first open at their base, become closed by extension of ossification inwards from each produced angle, converting the notch into a foramen. This forms a wide oval, the apex being produced into a long spine; but towards the end of the tail the spine becomes shortened, and the hæmal arch is reduced to a mere flattened ring. The size of the canal for the protection of the caudal blood-vessels indicates the powerful muscular actions of that part; as the produced spines from both neural and hæmal arches bespeak the provision made for muscular attachments, and the vertical development of the caudal swimming organ. All these modifications of the vertebral column demonstrate the aquatic habits of the Archegosaurus; the limbs being in like manner modified as fins, but so small and feeble as to leave the main part of the function of swimming to be performed, as in fishes and perennibranchiate batrachia, by the tail. The skull of the Archegosaurus appears to have retained much of its primary cartilage internally, and ossification to have been chiefly active at the surface; where, as in the combined dermo-neural ossifications of the skull in the sturgeons and salamandroid fishes, *e.g.*, Polypterus, Amia, Lepidosteus, these ossifications have started from centres more numerous than those of the true vertebral system in the skull of Saurian reptiles. The teeth are usually shed alternately. They consist of osteo-dentine, dentine, and cement. The first substance occupies the centre, the last covers the superficies of the tooth, but is introduced into its substance by many concentric folds extending along the basal half. These folds are indicated by fine longitudinal straight striæ along that half of the crown. The section of the tooth at that part gives the same structure which is shown by a like section of a tooth of the *Lepidosteus oxyurus*.[†] The same principle of dental composition is exemplified in the teeth of most of the ganoid fishes of the Carboniferous and Devonian systems, and is carried out to a great and beautiful degree of complication in the old red Dendrodonts. The repetition of the same principle of dental structure in one of the earliest genera of Reptilia, associated with the defect of ossification of the endo-skelton and the excess of ossification in the exa-skeleton of the head, decisively illustrate the true affinities and low position in the Reptilian class of the so-called Archegosauri. For other details of the peculiar and interesting structure of the animals representing the earliest or oldest known order of Reptiles, Professor Owen referred to the article "Palæontology" in the "Encyclopædia Britannica." This order is "carboniferous."

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* Reptilian aus der Steinkohlen Formation in Deutschland. Sechster Band, p. 61.

Order II. LABYRINTHODONTIA.—Head defended, as in the Ganocephala, by a continuous casque of externally sculptured and unusually hard and polished osseous plates, including the supplementary "postorbital" and "supertemporal" bones, but leaving a "foramen parietale."[‡] Two occipital condyles. Vomer divided and dentigerous. Two nostrils. Vertebral centra, as well as arches, ossified, biconcave. Pleurapophyses of the trunk, long and bent. Teeth rendered complex by undulation and side branches of the converging folds of cement, whence the name of the order. Osseous scutes in some. The reptiles presenting the above characters have been divided according to minor modifications exemplified by the form and proportions of the skull, by the relative position and size of orbital, nasal, and temporal cavities, &c., into the several genera; as *e.g.* Mastodonsaurus, Trematosaurus, Metopias, Capitosaurus, Zygosaurus, Xestorrhytias. The relation of these remarkable reptiles to the Saurian order has been advocated as being one of close and true affinity, chiefly on the character of the extent of ossification of the skull and of the outward sculpturing of the cranial bones. But the true nature of some of these bones appears to have been overlooked, and the gaze of research for analogous structures has been too exclusively upward. If directed downward from the Labyrinthodontia to the Ganocephali, and to certain ganoid fishes, it suggests other conclusions, which had been worked out by Professor Owen, in his article on "Palæontology," above referred to. There is nothing in the known structure of the so-named Archegosaurus or Mastodonsaurus that truly indicates a belonging to the Saurian or Crocodilian order of reptiles. The exterior ossifications of the skull and the canine-shaped labyrinthic teeth, are both examples of the Salamandroid modification of the ganoid type of fishes. The small proportion of the fore-limb of the Mystriosaurus in nowise illustrates this alleged Saurian affinity: for though it be as short as in Archegosaurus, it is as perfectly constructed as in the crocodile, whereas the short fore-limb of Archegosaurus is constructed after the simple type of that of the Proteus and Siren. But the futility of this argument of the sauroid affinities is made manifest by the proportions of the hind-limb of Archegosaurus; it is as stunted as the fore-limb. In the Labyrinthodonts it presented larger proportions, which, however, may be illustrated as naturally by these proportions of the limbs in certain Batrachia, as in the Teleosaurus.

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[*] Principal Forms of the Skeleton, Orr's "Circle of the Sciences," p. 187, § 11.

Wyman, "American Journal of the Natural Sciences," October, 1843.

The corresponding vacuity is larger in some ganoid fishes.

Order III. ICHTHYOPTERYGIA.—The bones of the head still include the supplementary "post-orbitals" and "supra-temporals," but there are small temporal and other vacuities between the cranial bones; a "foramen parietale," a single convex occipital condyle,* and one vomer which is edentulous. Two antorbital nostrils. Vertebral centra, ossified biconcave. Pleurapophyses of the trunk long and bent, the anterior ones with bifurcate heads. Teeth with converging folds of cement at their base; implanted in a common alveolar groove, and confined to the maxillary, premaxillary, and premandibular bones. Premaxillaries much exceeding the maxillaries in size. Orbit very large; a circle of sclerotic plates. Limbs natatory; with more than five multi-articulate digits; no sacrum. With the retention of characters which indicate, as in the preceding orders, an affinity to the higher Ganoidea, the present exclusively marine reptilia more directly exemplify the Ichthyic type in the proportions of the premaxillary and maxillary bones; in the shortness and great number of the biconcave vertebræ; in the length of the pleurapophyses of the vertebræ near the head; in the large proportional size of the eyeball, and its well-ossified sclerotic coat; and especially in the structure of the pectoral and ventral fins. The skin is naked. The order ranges from the lias to the chalk.

Order IV. SAUROPTERYGIA.—No post-orbital and supra-temporal bones:† large temporal and other vacuities between certain cranial bones; a foramen parietale; two antorbital nostrils; teeth simple, in distinct sockets of premaxillary, maxillary, and premandibular bones, rarely on the palatine or pterygoid bones; maxillaries larger than premaxillaries. Limbs natatory; not more than five digits. A sacrum of one or two vertebræ for the attachment of the pelvic arch in some, numerous cervical vertebræ in most. Pleurapophyses with simple heads; those of the trunk long and bent. In the Pliosaurus the neck vertebræ are comparatively few in number, short and flat. The sauropterygian type seems to have attained its maximum dimensions in this genus; the species of which are peculiar to the Oxfordian and Kimmeridgian divisions of the Upper Oolitic system. M. von Meyer regards the number of cervical vertebræ, and the length of neck, as characters of prime importance in the classification of Reptilia, and founds thereon his Order called Macrotrachelen, in which he includes Simosaurus, Pistosaurus, and Nothosaurus, with Plesiosaurus. No doubt the number of vertebræ in the same skeleton bears a certain relation to ordinal groups; the Ophidia find a common character therein; yet it is not their essential character; for the snake-like form, dependent on multiplied vertebræ, characterises equally certain Batrachians (Cæcilia) and fishes (Muræna). Certain regions of the vertebral column are the seats of great varieties in the same natural group of Reptilia. We have long-tailed and short-tailed lizards; but do not, therefore, separate those with numerous caudal vertebræ, as "Macrourau," from those with few or none. The extinct Dolichosaurus of the Kentish chalk, with its procœlian vertebræ, cannot be ordinally separated, by reason of its more numerous cervical vertebræ, from other shorter-necked procœlian lizards. As little can we separate the short-necked and the big-headed amphicœlian Pliosaur from the Macrotrachelians of Von Meyer, with which it has its most intimate and true affinities. There is much reason, indeed, to suspect that some of the Muschelkalk Saurians, which are as closely allied to Nothosaurus as Pliosaurus is to Plesiosaurus, may have presented analogous modifications in the numbers and proportions of the cervical vertebræ. It is hardly possible to contemplate the broad and short-snouted skull of the Simosaurus, with its proportionably large teeth, without inferring that such a head must have been supported by a shorter and more powerful neck than that which bore the long and slender head of the Nothosaurus or Pistosaurus. The like inference is more strongly impressed upon the mind by the skull of the Placodus, still shorter and broader than that of Simosaurus, and with vastly larger teeth, of a shape indicative of their adaptation to crushing molluscous or crustaceous shells. Neither the proportions and armature of the skull of Placodus, nor the mode of obtaining the food indicated by its cranial and dental characters, permit the supposition that the head was supported by other than a comparatively short and strong neck. Yet the composition of the skull, its proportions, cavities, and other light-giving anatomical characters, all bespeak the close essential relationship of Placodus to Simosaurus and other so-called Macrotrachelian reptiles of the Muschelkalk beds. Professor Owen continued, therefore, as in his Report of 1841, to regard the fin-like modification of the limbs as a better ordinal character than the number of vertebræ in any particular region of the spine. Yet this limb-character is subordinate to the characters derived from the structure of the skull and of the teeth. If, therefore, the general term Enaliosauria may be sometimes found convenient in its application to the natatory group of Saurian Reptiles, the essential distinctness of the orders Sauropterygii and Ichthyopterygii, typified by the Ichthyosaurus and Plesiosaurus respectively, should be borne in mind. The Plesiosaurus, with its very numerous cervical vertebræ, sometimes thirty in number, may be regarded as the type of the Sauropterygii, or pentadactyle sea-lizards. Of all existing reptiles, the lizards, and, amongst these, the Old World monitors (*Varanus*, Fitz.), by reason of the cranial vacuities in front of the orbits, most resemble the Plesiosaur in the structure of the skull; as in the division of the nostrils, the vacuities in the occipital region between the exoccipitals and tympanics, the parietal foramen, the zygomatic extension of the post-frontal, the palato-maxillary, and pterygosphenoid vacuities in the bony palate; and all these are lacersian characters as contradistinguished from crocodilian ones. But the antorbital vacuities, between the nasal, prefrontal, and maxillary bones, are the sole external nostrils in the Plesiosaurs. The zygomatic arch abuts against the fore part of the tympanic, and fixes it: a much greater extent of the roof of the mouth is ossified than in lizards, and the palato-maxillary and pterygo-sphenoid fissures are reduced to small size. The teeth, finally, are implanted in distinct sockets. That the Plesiosaur had the "head of a lizard" is an emphatic mode of expressing the amount of resemblance in their cranial conformation. The crocodilian affinities, however, are not confined to the teeth, but are exemplified in some particulars of the structure of the skull itself. In the simple mode of articulation of the ribs the lacertian affinity is again strongly manifested; but to this vertebral character such affinity is limited. All the others exemplify the ordinal distinction of the Plesiosaurs, from known existing reptiles. The shape of the ints of the centra; the number of vertebræ between the head and tail, especially of those of the neck; the slight indication of the sacral vertebræ; the non-confluence of the caudal hæmapophyses with each other,—are all "plesiosauroid." In the size and number of abdominal ribs and sternum may perhaps be discerned a first step in that series of development of the hæmapophyses of the trunk, which reaches its maximum in the plastron of the Chelonia. The connection of the clavicle with the scapula is common to the Chelonia with the Plesiosauri; the expansion of the coracoids—extreme in Plesiosauri—is greater in Chelonia than in Crocodilia, but is still greater in some Lacertia. The form and proportions of the pubis and ischium, as compared with the ilium, in the pelvic arch of the Plesiosauri, find their nearest approach in the pelvis of marine Chelonia; and no other existing reptile now offers so near, although it be so remote, a resemblance to the structure of the paddles of the Plesiosaur. Both Nothosaurus and Pistosaurus had many neck-vertebræ; and the transition from these to the dorsal series was affected, as in Plesiosaurus, by the ascent of the rib-surface from the centrum to the neurapophysis; but the surface, when divided between the two elements, projected further outwards than in most Plesiosauri. In both Nothosaurus and Pistosaurus the pelvic vertebræ develop a combined process (par- and di-apophysis), but of relatively larger, vertically longer size, standing well out, and from near the fore-part of the side of the vertebra. This process with the coalesced riblet indicates a stronger ilium, and a firmer base of attachment of the hind-limb to the trunk than in Plesiosaurus. Both this structure and the greater length of the bones of the fore arm and leg show that the Muschelkalk predecessors of the liassic Pleisosauri were better organised for occasional progression on dry land. The Sauropterygii extend from the Trias to the chalk inclusive.

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* This character is retained throughout the rest of the class, save in Batrachia, and will not afterwards be expressed in their characters.

† These bones do not reappear in the subsequent orders.

Order V. ANOMODONTIA (*ανομος*, lawless, *οδους*, tooth).—This order is represented by three families, all the species of which are extinct, and appear to have been restricted to the triassic period. Teeth wanting, or confluent with tusk-shaped premaxillaries, or confined to a single pair in the upper jaw, which have the form and proportions of canine tusks. A foramen parietale and two nostrils, tympanic pedicle fixed. Vertebræ biconcave; pleurapophysis of the trunk long and curved, the anterior ones with bifurcate heads; a sacrum of four or five vertebræ forming, with broad iliac and pubic bones, a large pelvis. Limbs, ambulatory.—Family, Dicynodontis. A long, ever-growing tusk in each maxillary bone; premaxillaries connate, and forming with the lower jaw a beak-shaped mouth, probably sheathed with horn. This includes two genera,—Dicynodon and Ptychognathus,—all the known species of which are founded on fossils from rocks of probably triassic age in South Africa.—Family Cryptodontia. Upper as well as lower jaw edentulous. The genus Oudenodon closely conforms to the dicynodont type, and the species are from the same rocks and localities.—Family Gnathodontia. Two curved tusk-shaped bodies holding the place of the premaxillaries, and consisting of confluent dentinal and osseous substance, descending in front of the symphysis mandibulæ. These bodies are homologous with the pair of confluent premaxillary teeth and bones in the existing New Zealand amphicœlian lizard Rhynchocephalus; they are analogous to the tusks in the Dicynodonts, and must have served a similar purpose in the extinct reptiles of the new red (Trias) sandstone of Shropshire (Rhynchosaurus), in which alone this structure, with an otherwise edentulous beak-shaped mouth, has hitherto been met with. To this order belongs the Rhynchosauroid reptile, from the Elgin sandstone, with palatal teeth, called Hyperodapedon by Professor Huxley.

Order VI. Pterosaurus.—Although some members of the preceding Order resembled birds in the shape or the edentulous state of the mouth, the reptiles of the present order make a closer approach to the feathered class in the texture and pneumatic character of most of the bones, and in the modification of the pectoral limbs for the function of flight. This is due to the elongation of the antibrachial bones, and more especially to the still greater length of the metacarpal and phalangial bones of the fifth or outermost digit, the last phalanx of which terminates in a point. The other fingers were of more ordinary length and size, and were terminated by claws, the number of their phalanges progressively increasing to the fourth, which had four joints. The whole osseous system is modified in accordance with the possession of wings; the bones are light, hollow, most of them permeated by air-cells, with thin, compact outer walls. The scapula and coronoid are long and narrow, but strong. The vertebræ of the neck are few, but large and strong,—for the support of a large head with long jaws, armed with sharp-pointed teeth. The skull was lightened by large vacuities, of which one was interposed between the nostril and the orbit. The vertebræ of the back are small—as are those of the sacrum, which were from two to five in number—but combined with a small pelvis and weak hind limbs, bespeaking a creature unable to stand and walk like a bird; the body must have been dragged along the ground like that of a bat. The vertebral bodies were united by ball-and-socket joints, the cup being anterior, and in them we have the earliest manifestation of the "procœlian" type of vertebra. The Pterosauria are distributed into genera according to modifications of the jaws and teeth. In the oldest known species, from the lias, the teeth are of two kinds: a few, at the fore-part of the jaws, are long, large, sharp-pointed, with a full elliptical base, in distinct and separated sockets; behind them is a close-set row of short, compressed, very small, lancet-shaped teeth. These form the genus Dimorphodon, Ow. In the genus Ramphorhynchus, V. M., the fore-part of each jaw is without teeth, and may have been incased by a horny beak; but behind the edentulous production there are four or five large and long teeth followed by several smaller ones. The tail is long, stiff, and slender. In the genus Pterodactylus, Cuv., the jaws are provided with teeth to their extremities; all the teeth are long, slender, sharp-pointed, set well apart. The tail is very short. *P. longirostris*, Ok., about ten inches in length. From lithographic slate at Pappenheim, *P. crassirostris*, Goldf., about one foot long, and *P. Sedgwickii*, Ow., from the greensand, with an expanse of wing of twenty feet, exemplify the Pterodactyles proper. The oldest well-known Pterodactyle is the *Dimorphodon macronyx* of the lower lias; but bones of Pterodactyle have been discovered in coeval lias of Wirtemberg. The next in point of age is the *Dimorphodon Banthensis*, from the "Posidonomyen-Shiefer" of Banz in Bavaria, answering to the alum shale of the Whitby lias. Then follows the *P. Bucklandi*, from the Stonesfield oolite. Above this come the first-defined and numerous species of Pterodactyle from the lithographic slates of the middle oolitic system in Germany, and from Cirin on the Rhone. The Pterodactyles of the Wealden are, as yet, known to us by only a few bones and bone fragments. The largest known species are the *P. Sedgwickii* and *P. Fittoni*, from the upper greensand of Cambridgeshire. Finally, the Pterodactyles of the middle chalk of Kent, almost as remarkable for their great size, constitute the last forms of flying reptile known in the history of the crust of this earth.

Order VII. Thecodontia.—Vertebral bodies biconcave; ribs of the trunk long and bent, the anterior ones with a bifurcate head; sacrum of three vertebræ; limbs ambulatory, femur with a third trochanter. Teeth with the crown more or less compressed, pointed, with trenchant and finely serrate margins; implanted in distinct sockets. This order is represented by the extinct genera Thecodontosaurus and Palæosaurus of Riley and Stutchbury, from probably triassic strata, near Bristol; by the Cladyodon of the new red sandstone of Warwickshire, with which, probably, the Belodon of the Keuper sandstone of Wirtemberg is generically synonymous. The Bathygnathus, Leidy, from new red sandstone of Prince Edward's Island, North America, is probably a member of the present order, which seems to have been the forerunner of the next.

Order VIII. Dinosauria.—Cervical and anterior dorsal vertebræ, with par- and di-apophyses, articulating with bifurcate ribs: dorsal vertebræ with a neutral platform; sacral vertebræ from four to six in number. Articular ends of the free vertebræ, more or less flat; but in the cervical becoming convex in front and concave behind, in some species. Limbs ambulatory, strong, long, and unguiculate. Femurwith a third trochanter in some. The species of this order were of large bulk, and were eminently adapted for terrestrial life; some, *e.g.*, Iguanodon and, probably, Hylæosaurus, were more or less vegetable feeders; others, *e.g.*, Megalosaurus, were carnivorous. The Dinosauria ranged, in time, from the lias (Scelidosaurus, Ow., from Charmouth) to the upper greensand (Iguanodon). The Megalosaurus occurs in the lower oolite to the Wealden inclusive. The latter formation is that in which the Dinosauria appear to have flourished in greatest numbers and of hugest dimensions.

Order IX. Crocodilia.—Teeth in a single row, implanted in distinct sockets, external nostril single and terminal or subterminal. Anterior trunk; vertebræ with par- and di-apophyses, and bifurcate ribs; sacral vertebræ two, each supporting its own neural arch. Skin protected by bony, usually pitted, plates.

Sub-Order, *Amphicœlia* (*αμφι*, both; *κοιλος*, hollow; the vertebræ being hollowed at both ends).—Crocodiles, closely resembling in genera, form the long and slender-jawed kind of the Ganges, called Gavial, existed from the time of the deposition of the lower lias. The teeth of the liassic forms were similarly long, slender, and sharp, adapted for the prehension of fishes, and their skeleton was modified for more efficient progress in water, by both the terminal vertebral surfaces being slightly concave, by the hind limbs being relatively larger and stronger, and by the orbits forming no prominent obstruction to progress through water. From the nature of the deposits containing the remains of the so-modified crocodiles they were marine. The fossil crocodile from the Whitby lias, described and figured in the *Philosophical Transactions*, 1758, p. 688, is the type of these Amphicœlian species. They have been grouped under the following generic heads:—Teleosaurus, Mystriosaurus, Macrospondylus, Massospondylus, Pelagosaurus, Aeolodon, Suchosaurus, Goniopholis, Pœcilopleuron, Stagonolepis (?), &c.* Species of the above genera range from the lias to the chalk inclusive.

Sub-Order Opisthocœlia (οπισθος, behind; κοιλος, hollow; vertebræ concave behind, convex in front).—The small group of crocodilia, so called, is an artificial one, based upon more or less of the anterior trunk vertebræ, being united by ball-and-socket joints, but having the ball in front, instead of, as in modern crocodiles, behind. Cuvier first pointed out this peculiarity† in a crocodilian from the Oxfordian beds at Harfleur and the Kimmeridgian at Havre. Professor Owen had described similar Opisthocœlian vertebræ from the great oolite at Chipping Norton, from the upper lias of Whitby, and, but of much larger size, from the Wealden formations of Sussex and the Isle of Wight. These specimens probably belonged, as suggested by him in 1841,‡ to the fore-part of the same vertebral column as the vertebræ, flat at the fore-part, and slightly hollow behind, on which he founded the genus Cetiosaurus. The smaller Opisthocœlian vertebræ described by Cuvier have been referred by Von Meyer to a genus called Streptospondylus. In one species, from the Wealden, dorsal vertebræ, measuring 8 inches across, are only 4 inches in length, and caudal vertebræ nearly 7 inches across are less than 4 inches in length. These characterise the species called *Cetiosaurus brevis.* Caudal vertebræ, measuring 7 inches deep and 5½ inches in length, from the lower oolite at Chipping Norton, and the great oolite at Enstone, represent the species called *Cetiosaurus medius.* Caudal vertebræ from the Portland stone at Garsington, Oxfordshire, measuring 7 inches 9 lines across, and 7 inches in length, were referred by the author to the *Cetiosaurus longus.* The latter, he remarked, must have been the most gigantic of crocodilians.

Sub-Order. Procœlia (προς, front; κοιλος, hollow; vertebræ with the cup at the fore-part and the ball behind). Crocodilians with cup-and-ball vertebræ, like those of living species, first make their appearance in the greensand of N. America (*Crocodilus basifissus* and *C. basitruncatus*, Ow.)§ In Europe, their remains are first found in the tertiary strata. Such remains from the plastic clay of Meudon have been referred to *Crocodilus isorhynchus*, *C. cœlorhyncus*, *C. Becquereli.* In the "Calcaire Grossier" of Argenton and Castelnaudry have been found the *C. Rallinati*, and *C. Dodanii.* In the coeval eocene London clay, at Sheppy Island, the entire skull and characteristic parts of the skeleton of *C. toliapicus* and *C. champsoïdes* occur. In the somewhat later eocene beds at Bracklesham occur the remains of the Gavial-like *C. Dixoni.* In the Hordle beds have been found the *C. Hastingsiæ*, with short and broad jaws; and also a true alligator (*C. Hantoniensis*). It is remarkable that forms of procœlian Crocodilia, now geographically restricted, the gavial to Asia, and the alligator to America, should have been associated with true crocodiles, and represented by species which lived, during nearly the same geological period, in rivers flowing over what now forms the south coast of England. Many species of procœlian Crocodilia have been founded on fossils from miocene and pliocene tertiaries. One of these, of the gavial sub-genus (*C. Crassidens*), from the Sewalik tertiary, was of gigantic dimensions.

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* This was referred to the present order, by the author, after inspection of the specimens brought to the British Association Meeting at Leeds, by Sir R. Murchison, but with a note on the greater relative breadth of the coracoid, as shown by the part of the bone then exposed.—(Encyclo. Brit., Art. "Palæontology.") Prof. Huxley, to whom the specimens were subsequently consigned for description, together with others directly transmitted to him, confirms the general crocodilian character of Stagonolepis. I regard the modifications of the limb-bones as indications of affinity with the Thecodontia; but the structure of the cranium must be ascertained to determine this point. The associated fossils, especially those allied to Rynchosaurus, in the Elgin sandstones, have a triassic character.

† Annales du Muséum, tom. xii. p. 83, pl. x., xi.

‡ Report on British Fossil Reptiles, *Trans. British Association* for 1841, p. 96.

§ Quarterly Journal of the Geological Society.

Order X. Lacertilia.—Vertebræ, in most, procœlian, with a single transverse process on each side, and with single-headed ribs; sacral vertebræ, not exceeding two. Small vertebræ of this type have been found in the Wealden of Sussex. They are more abundant, and are associated with other and more characteristic parts of the species in the cretaceous strata. On such evidence have been based the *Rhaphiosaurus subulidens*, the *Coniasaurus crassidens*, and the *Dolichosaurus longicollis*. But the most remarkable and extreme modification of the lacertian type, in the cretaceous period, is that manifested by the huge species, of which a cranium, five feet long, was discovered in the upper chalk of St Peter's Mount, near Maestricht, in 1780. This species, under the name Mosasaurus, is well known by the descriptions of Cuvier. Allied species have been found in the cretaceous strata of England and North America. The Leiodon anceps of the Norfolk chalk was a nearly allied marine Lacertian. The structure of the limbs is not yet well understood; it may lead to a subordinal separation of the Mosasauroids from the land-lizards, most of which are represented by existing species, in which a close transition is manifested to the next order.

Order XI. Ophidia.—Vertebræ very numerous, procœlian, with a single transverse process on each side; no sacrum; no visible limbs. The earliest evidence, at present, of this order is given by the fossil vertebræ of the large serpent (Palæophis, Ow.) from the London clay of Sheppy and Bracklesham. Remains of a poisonous serpent, apparently a Vipera, have been found in miocene deposits at Sansans, south of France. Ophidiolites, from Œningen, have been referred to the genus Coluber.

Order XII. Chelonia.—The characters of this order, including the extremely and peculiarly modified forms of tortoises, terrapenes, and turtles, are sufficiently well known. The chief modifications in oolitic Chelonia known to Professor Owen were the additional pair of bones, interposed between the hyosternals and the hyposternals of the plastron, in the genus Pleurosternon from the upper oolite at Purbeck. It would be very hazardous to infer the existence of reptiles, with the characteristic structure of the restricted genus Testudo, from the footprints in the triassic sandstone of Dumfriess-shire. But Professor Owen concurred in the general conclusions based upon the admirable figures and descriptions in the splendid monograph by Sir William Jardine, Bart., F.R.S., that some of those footprints most probably belonged to species of the Chelonian order. An enormous species of true turtle (*Chelone gigas*), the skull of which measured one foot across the back part, had left its remains in the eocene clay at Sheppy. The terrestrial type of the order had been exemplified on a still more gigantic scale by the Colossochelys of the Sewalik tertiaries.

Order XIII. Batrachia.—Vertebræ biconcave (*Siren*), procœlian (*Rana*), or opisthocœlian (*Pipa*); pleurapophyses short, straight. Two occipital condyles and two vomerine bones, in most dentigerous; no scales or scutes. Larvæ with gills, in most deciduous. Representatives of existing families or genera of true Batrachia have been found fossil, chiefly in tertiary and post-tertiary strata. Indications of a perennibranchiate batrachian had recently been detected by Professor Owen, in a collection of minute Purbeck fossils. Anourous genera (*Palæophrynus*), allied to the toad, occurred in the Œningen tertiaries, and here also the remains of the gigantic Salamander (*Andrias Schenchyeri*) were discovered.

Summary of the above defined Orders.

Province—VERTEBRATA.
Class—HÆMATOCRYA.
Sub-Class—REPTILIA.

Orders.

I. Ganocephala.
II. Labyrinthodontia.
III. Ichthyopterygia.
IV. Sauropterygia.
V. Anomodontia.
VI. Pterosauria.
VII. Thecodontia.
VIII. Dinosauria.
IX. Crocodilia.
X. Lacertilia.
XI. Ophidia.
XII. Chelonia.
XIII. Batrachia.

Professor HUXLEY thought this communication a most important contribution to science. He quite agreed with Professor Owen in placing together the Amphibia and fishes, as no real distinction could be drawn between them. It was, however, different with the true Reptiles and Amphibia, although Professor Owen was not disposed to attach importance to these distinctions. The Amphibia possessed no allantois and had gills, points of structure which separated them strongly from the true Reptiles. Amongst extinct animals none presented any transitional forms.—Professor OWEN defended his own position on the ground that such an interpretation could be given to the allantois on the one side and the gills on the other, as to render the distinctions less obvious than at first sight appeared.

On the Identity of Morrhua vulgaris (*the common Cod*) *and* Morrhua punctata (*the speckled Cod*), *hitherto described as distinct species.* By Dr DYCE.—The author of this paper showed that the distinction between these fishes consisted mainly in a diseased condition of the bones. This peculiarity existed in haddocks and other fishes. This condition of the bones consisted in an absorption of the centrum of the vertebræ, and resembled in its effects the disease called rickets in the human being.

On the Distribution of British Butterflies. By Mr H. T. STAINTON.—Among the insect tribes, the "scale-wings" or order Lepidoptera, has always attracted a considerable amount of attention. The variety and beauty of the butterfly tribe is a matter of notoriety. The order Lepidoptera includes two great divisions, butterflies and moths,—the former group all fly by day, whereas most of the moths are nocturnal in their habits. It has been calculated that there are not less than 50,000 different species of Lepidoptera on the globe. More than 3000 species of butterflies are already known, and it has been computed that the moths are sixteen times as numerous. In this country the proportion of moths is much greater, being nearly 30 to 1, but then we are remarkable throughout Europe for our poverty in butterflies. As already observed, in the whole world 3000 species of butterflies are already known; of these only one-tenth occur in Europe, the tropical parts of Asia and America being by far the most numerously populated with this beautiful tribe of insects. In central Europe or Germany, 186 species of butterflies have been observed, the remaining 120 European species being peculiar to

Spain, Italy, Greece, Russia, or Lapland. Of the German species 94 occur in Belgium, but only 65 in England, though we possess one species, *Erebia cassiope,* which does not occur in Belgium. All the British butterflies occur in England, but little more than half (only 33) are found in Scotland, and scarcely more in Ireland. Twenty-five species may be considered as generally distributed and common ; but it should not be understood that these are everywhere to be met with, but simply that their geographical range is not limited, and that where they find suitable localities we may expect to meet with them from Norfolk to Killarney, and from the Isle of Wight to Caithness,—some frequent gardens, some meadows, some heaths, some woods, and some hedgerows and lanes. Twenty-five other species, which all occur in the south-east of England, thin out as we advance northwards and westwards,—only five of them occurring in Scotland, only fourteen in Ireland. Three species, two of which are common in the mountainous parts of Scotland, do not occur at all in the south of England. Seven species are local to particular limited districts in the midland counties or the south of England. Three species of rare occurrence in this country must be looked upon as stragglers from the Continent; one of them, *Vanessa antiopa,* has occurred in the south-west of Scotland, at Dunbar, and at Jardine Hall, Dumfriesshire. Two other species, which formerly occurred in restricted English localities, now appear to be extinct there.

On the Employment of the Electrical Eel (Gymnotus electricus) *by the Natives of Surinam.* By Professor G. Wilson.—After alluding to the paper he had read at the last meeting of the Association, on the electrical Malapterurus from Old Calabar, the author gave an account of the employment of the Gymnotus in Surinam as a medicinal agent. He had obtained his information from a gentleman who had expressed his willingness to forward to England living specimens of this electrical fish for experiment.

On the Zoophytes of Caithness. By Mr C. W. Peach.—He commenced by extolling the utility of local catalogues of natural history, and stated that he was desirous of showing how rich the Scottish shores are in these lovely gems, in order that he might induce many to draw up these beauties from ocean's caves. He then mentioned Mr J. Macgillivray's list, the result of about three weeks' examination on the Aberdeen coast, as the only Scottish one we had—it contained 64 species; and then proceeded to compare his own with those of Couch's for Cornwall, and Alder's for Durham and Northumberland—the former contains 124 species, the latter 164; thus giving a preponderance of 40 species to Alder's. He enumerated in his own list 150 species, and thus a balance of 14 only is left against Caithness, &c. He believed this will soon be reduced when greater attention has been paid to the fresh-water ones, and the more obscure forms, and when the dredge has been used; for hitherto all had been collected between tide-marks, and from the refuse of the fishermen's lines, and all (with the exception of *Plumularia myriophyllum,* at Peterhead, by the Rev. Mr Yevill) by himself and sons: the greatest number of southern forms being found at Wick. A few forms found at Peterhead are wanting at Wick, and *vice versâ.*

On the Characteristic Features of the Aberdeenshire Flora. By Dr. Dickie.—Remarks on the physical characters of Aberdeenshire form a necessary introduction to an account of its Flora. The county of Aberdeen occupies a position between 56° 52′ and 57° 42′ N., and 1° 49′ to 3 48′ W. long.; it embraces a surface of 1950 English square miles. A line drawn from Culter, on the borders of Kincardineshire, to Pennan, on the borders of Banffshire, divides it into two portions, presenting very great difference in physical characters. To the east of this line the surface, though undulating, does not present any point exceeding 900 feet in elevation, and no part of this section is more than twenty miles distant from the German Ocean. The more inland part, to the west of the line above mentioned, has in general a very different aspect, there being a gradual rise of the surface towards the south-western extremity of the county. This is very obvious on tracing the levels of the two principal rivers the Dee and the Don. The former has an elevation of 1640 feet at a distance of seventy miles from the sea; the Don, about fifty-five miles inland, is 1240 feet above the sea. The river Muick, in a course of ten miles only, from its source at Loch Muick to its conjunction with the Dee at Ballater, presents a difference of level amounting to more than 500 feet. These facts are singularly in contrast with observations made on the course of the river Ythan, which drains part of the more eastern district: at twenty-two miles from its termination in the German Ocean, it is only 124 feet above the level of that sea. Some of the passes from one glen to another illustrate the same point: the highest level of the path on the east shoulder of Mount Battock, twenty-eight miles from Aberdeen, is about 2000 feet, while that on the west shoulder of Mount Keen, ten miles further inland, attains an elevation of 2400 feet. Again, if we take a general view of the heights of the mountains in sections of ten miles from east to west, we observe a steady increase of elevation, till we reach a zone in which few of the numerous mountains are lower than 2000 or 3000 feet, and many exceed 4000, the extreme elevation being that of Ben Muich Dhui, viz., about 4320 feet, and therefore, in Britain, second only to Ben Nevis. Omitting here other details respecting the shore-line, prevailing rocks and soil, temperature, rain, &c., the following is a summary of conclusions respecting the vegetation. Excluding upwards of forty species, many of which, though now extensively diffused, have doubtless been introduced at a comparatively recent period, the indigenous flowering plants amount to 635, consisting of 458 Dicotyledons and 177 Monocotyledons; these are distributed among 53 natural orders of the former and 11 of the latter. The Flora, therefore, is not rich as regards mere numbers; nevertheless it comprehends many species of great interest.

On a new Genus of Lucernariadæ. By Professor Allman.—This creature was a kind of fixed Medusa, having a structure resembling many of the common forms of floating Jelly Fishes, but was fixed to rocks by means of a pedicle or stalk. It had been found on the more northern shores of Scotland, and he proposed for it the name of *Carduella Scotica.*

On Drift Pebbles found in the Stomach of a Cow. By the Rev. W. S. Symonds.—Mr Symonds exhibited thirty pebbles, one of them weighing three-quarters of a pound, found in the stomach of a cow lately killed at Barton-under-Needwood, Burton-on-Trent. The pebbles belong to the northern drift of geologists, which abundantly overlies the new red sandstone of the district; and they are remarkably glazed and polished by the action of the cow's stomach. The weight of the pebbles is five pounds, and the animal appeared perfectly healthy and fat when killed by Mr Goodman, butcher, of Barton-under-Needwood, to whom reference may be made.

Short Account of a Bone Cave, near Montrose. By Mr Beattie.—The cave is situate near the mouth of the River Northesk, in that range of trap-rocks extending eastward from the Northwater Bridge, on the Aberdeen Road, to the cliffs of St Cyrus,—the base of the cave being at present ten or twelve feet above the level of the sea, from which it is distant nearly a mile, and from the nearest point of the River Northesk about half as much. The entrance to the cave is through a hard compact rock of trap, and measures twelve feet wide, by five high. On entering, the cavity suddenly widens out to the breadth of twenty feet, with a height varying from twenty to thirty,—the whole having been crammed to the roof with a deposit of fine dark loamy soil, containing a variety of organic remains. It was evident that the work of excavation had been carried on for some time, and we discovered evidences on Mr Walker's farm that to him the cave had proved a regular bed of guano, fertilising his soil and improving his crops. In his operations, however, many of the fossil remains had been allowed to be taken away; still the almost perpendicular section left standing afforded ample field for inquiry and speculation. The bottom, or floor, consisted of rolled stones, or sea-beach, in some places mixed, or covered, with stalagmitic concretion several inches thick. The lowest stratum, three feet thick, was composed of dark loam, with a mixture of decayed shells, principally of the *Mytilus edulis*. Above this, extending round the cave, was a remarkable layer of shells of the *Patella vulgata*, varying from one to three feet deep, all in the finest possible state of preservation, and of a large size,—many of them measuring upwards of two inches across. This extraordinary deposit of shells contained no admixture of sand or earthy matter, but lay pure and clean, as if heaped together by human agency. A few examples of *Turbo littoreus* of Linn. were picked up. About eight feet from the floor, we found a stratum of decayed animal matter, about a foot deep, with a layer of bones extending throughout the whole width of the cave. The teeth and bones were discovered in this layer, and, so far as yet observed, they belong chiefly to the Ruminantia, and are very similar to some of those from the Kirkdale Cave, represented in the plates to Buckland's "Reliquiæ Diluvianæ," especially the Deer horns figured in plate ix., 2d edition. The whole of the bones have been shattered, except the joints and other solid parts; on these we perceived marks, as if they had been gnawed by some animal. The only examples of Carnivora yet met with are the head of a wild cat and the jaws of a fox or wolf, with teeth belonging to animals of a larger species. About a foot from the floor we turned up part of the left parietal bone of a human skull, extremely thin, but compact, firm, and smooth as a piece of ivory. No other part of the human subject had been found, so far as our investigation proceeded. Two small pieces of a pipkin were also picked up, bearing evident marks of antiquity. The floor of the cave dips inward at an angle of about 10 degrees to the horizon, which leads to the supposition that there is a connection with some other cavern into which the sea has had access by this opening; or that another cave had existed between it and the sea, through which the shells might have been carried to their present position. It is not improbable that another cave may be found a little to the west of the present, where the rock is hidden by the débris from above and the soil that has fallen from the upper grounds. Speculation on this subject at present would be idle, but we cannot refrain from alluding to the marked similarity which exists between the remains found in this cave and those found in that of Kirkdale—the natural inference from which leads us to suppose that this also was a hyæna cave, and that remains of this animal may be found on further search being made; for although no bones of any carnivorous animal larger than the wolf have yet been found, it must be kept in mind that no remains of the hyæna were met with in the Kirkdale Cave for nearly twelve months after its discovery, and then only by chance.

Professor Owen remarked that the bones and shells from Montrose were those of recent animals, and that the cave had evidently been filled in a comparatively recent period.

On the Varieties and Species of New Pheasants recently introduced into England. By Mr Gould.—After a sketch of the distribution of the family of Gallinaceous birds, the author gave an account of the species of Phasianus (Pheasant), which had been introduced into England. All the species were from Asia. The oldest English species was the *P. Colchicus*, which came from Asia Minor. The next was *P. torquatus*, from Shanghai, which was introduced about one hundred years ago, and had recently been reintroduced. Specimens of this kind reared in Bedfordshire were exhibited. The crosses between these two birds produced remarkably fine and strong birds. The other true species were *P. Mongolicus* from Mongolia, *P. Læmmenigii* from Japan, *P. Reevesii* from China, and *P. versicolor* from Japan. *P. Reevesii* is remarkable for a tail six feet in length; whilst the last species had been successfully introduced into England, and bred freely with *P. Colchicus*, and the crosses between that bird and *P. torquatus*; and the result had been greatly to improve the strength and weight of the birds.

On the Vegetative Axis of Ferns. By Dr Ogilvie.—The paper embraced two principal points,—the general form of the Rhizome of Ferns, and its internal structure. The stems of our British species, at least, may be reduced to two forms,—the creeping Rhizome and the Caudex, branched or simple. We have examples of the first in our Brackens and Polypodies, and of the others in the tufted stem of Blechnum and Osmunda, the lady-fern and its congeners, and the parsley-fern, and in the massive imbricated root-stock of the male fern and some other species of Aspidium. The last form presents many points of similarity to the stem of a tree-fern, though its small development and horizontal line of growth prevent its forming any conspicuous trunk above the surface of the ground. The resemblance becomes more apparent when the persistent basis of the decayed fronds are cut off, and only the central axis left, marked by spiral rows of cicatrices like the scars marking the stem of the tree-fern. The chief peculiarity of the *internal* structure is the reduction of the fibro-vascular system to a netted cylinder, imbedded in the general cellular tissue of the stem, and giving off fasciculi both to the petioles and the rootlets. This arrangement is very regular in all the species, but there is great diversity in the course of the dark-coloured or woody tissue. Reference was made to the independent origin of the rootlets, and to the general relations of this form of stem to those of the higher plants.—The paper was illustrated by diagrams, and by preparations and dissections of our indigenous ferns, with some comparative specimens of the arborescent species.

MECHANICAL SCIENCE.

On Experiments to determine the Efficacy of continuous and Self-acting Breaks for Railway Trains. By Mr W. Fairbairn.—Of late years, Mr Fairbairn remarked, the improvements introduced to diminish the danger of railway travelling have been specially directed to increasing the retarding power of various kinds of breaks. The importance has been felt of reducing the momentum of trains with ease and rapidity,—that is, in the least time, and in the shortest distance. On this subject a most important communication had been made to the Railway Department of the Board of Trade by Colonel Yolland, who had experimented with breaks which were improvements on the ordinary breaks. The breaks used were the steam-break of M'Connell, the continuous break of Fay, the self-acting break of Newall, and the self-acting buffer-break of Guerin. Colonel Yolland had reported in favour of Newall's break for heavy traffic, and also in favour of that of Guerin under certain circumstances. Similar experiments had been carried out by Mr Fairbairn on the Lancashire and Yorkshire Railways. The breaks he used were those of Fay and Newall, and consisted of break-blocks, acting on every wheel of the carriages of the whole train—the break-blocks being suspended on flaps or placed on side-bars under the carriages. Powerful springs had also been applied under each carriage, by means of which the breaks were made to act instantaneously throughout the whole train by the act of one guard only, and this was one of the most important features of these breaks. The trains passed over a measured distance by the action of gravity. The trains employed consisted of three weighted carriages each. They were started by removing a stop. Having descended a previously measured distance with a uniformly accelerating velocity, they passed over a detonating signal, which gave notice to the guard to put on the break. On making experiments at Southport, a retarding force per ton weight was gained of 382·6 lb. for Newall's break, and 406·4 lb. for Fay's. The general result of the whole experiment showed that a train could be stopped by these breaks at a velocity of 20 miles an hour in 23·4 yards; 40 miles an hour in 93·8 yards; 50 miles an hour in 146·8; and 60 miles an hour in 211·5 yards. This clearly showed the advantage of these breaks in power.

On the result of Boring for Water in the New Red Sandstone, near Shiffnal, in the County of Salop. By Mr J. F. Bateman.—The supply of water to Wolverhampton being found insufficient, new works have been constructed by the author for bringing the water from the River Worth, nine miles from Wolverhampton, and three from Shiffnal. The River Worth, at the place where the pumping-works are erected, is not more than forty or fifty feet above the Severn, which it joins at Bridgewater, eight or ten miles distant. It may therefore be considered as the bottom of a basin little above the level of the sea. From the character of the surrounding hills, and the inclination of the beds of new red sandstone, it appeared to the author of the paper likely, that although the wells previously sunk on the high plateau of Wolverhampton had proved comparative failures, a considerable quantity of water might be found in the sandstone at the lower level, and that some might overflow, as an artesian well. A bore-well was accordingly commenced near Shiffnal, 12 inches in diameter, and continued for 70 feet, when it was diminished to 7 inches, and carried down to a total depth of 260 feet from the surface. Water was met with first at a depth of 22 feet, and from that time it rose with increasing supply to the surface, and flowed over as an artesian well, giving a supply in the end of 210,000 gallons daily. Throughout the whole depth of boring the work varied little in character. It was nearly all hard rock, sometimes very hard, with occasional beds of soft stone. For the last 40 feet or so the soft beds were thicker; but otherwise there was little change from top to bottom. As the whole well is charged with water to the level of the river, which forms its natural outlet, and as the boring shows that the lower beds receive their supplies from distant sources, the supply may reasonably be expected to be inexhaustible within the limits of that which is due to the percolation of the rain upon the collecting area.

Description of the Granite Quarries of Aberdeen and Kincardineshire. By Mr A. Gibb.—The working of the quarries in Aberdeen commenced 250 years ago; but little progress was made for 100 years. The houses in Aberdeen were constructed principally of wood till 1741, when a fire taking place, the town-council ordained that the fronts of the houses should be of stone or brick. In 1764 granite was recommended for paving the streets of London, and was used for Waterloo Bridge in 1817, and subsequently for the docks at Sheerness and London Bridge. There are upwards of twenty quarries supplying the different varieties of granite—the blue, the red or Peterhead granite, the light red, soft gray, and white. The granite, for the most part, lies in irregular masses in the quarries, and generally of columnar structure. The quarrying is principally carried on by blasting. The drainage of the quarries is chiefly accomplished by means of siphons of lead-pipe, from 1 to 2 or 3 inches in diameter. The author suggests the use of a locomotive engine on rails for drainage purposes, as well as for crane and lifting work. The quarries are not worked to any great depth, though the best and largest masses are found at the

lower depths; and proper mechanical contrivances for working deeper might be used with advantage. With reference to the durability of the granite, there appears no appreciable decay; on the oldest specimens of several hundred years the tool-marks are as sharp and fresh as at first. The tools used in dressing the granite for a long period were hammers, picks, and axes only; but in 1820 steel chisels were introduced, which effected a considerable improvement. Machinery was tried for dressing, but it failed, being in the form of a planing-machine, the granite requiring a distinct blow to separate the parts. The number of workmen employed in the quarries is about 500 daily, and the number of horses about 50. About 50,000 tons are quarried annually, of which about 30,000 are exported; and the export is increasing at the rate of 500 tons annually.

Royal Society of Edinburgh.

Monday, 5*th December* 1859.

At the request of the Council, Lord Neaves, V.P., delivered the following Opening Address:—

It has been customary for those who have opened the business of the Session in the Royal Society, from the seat which I now occupy, to give some notice of those members who may have been taken from us by death during the preceding year. The rolls of the Society still exhibit many names illustrious both in science and in literature, but seldom has a year occurred in which we have been deprived of so great a number of eminent members. The first whom I shall mention is Principal Lee:—

John Lee, late Principal of the University of Edinburgh, was one of the most remarkable and estimable men of his time. His intellectual qualities were of a high order; his attainments and acquisitions of knowledge were of the most varied and extensive kind. On almost all subjects he was admirably well informed, and in some departments he was unquestionably the most learned man of his age and country. He was more than all this: he was a most pious Christian minister, and he was one of the most friendly and affectionate of men.

Dr Lee was born at Torwood-lee-Mains, in the parish of Stowe, on the 22d of November 1779. He received his early education from the care of his mother, whom he was accustomed to speak of as a woman of remarkable intellectual powers and mental cultivation, as well as of distinguished moral excellence. The debt of gratitude which he owed to his parents must indeed have been great, if it bore any proportion to the filial reverence and devotion which he showed them in every form in after life.

He was sent, when a boy of ten years old, to Cadon Lee School, at Clovenford, then taught by Mr James Paris, and in which, during Dr Lee's attendance, Doctor Leyden was an assistant. From that school he went to the University of Edinburgh in 1794, being then in his fifteenth year. In his opening address to the University, as Principal, in 1842, he refers to its state when he became a student, and recurs with pride and pleasure to the eminent men who then gave and received instruction within its walls. He continued at the University for ten years, having studied both medicine and theology. He took the degree of M.D. in 1801, when his Graduation Thesis was much admired for its Ciceronian Latinity. He was licensed as a probationer of the Church in 1804.

During his attendance at college, he assisted Professor Robison in editing Dr Black's "Lectures on Chemistry." In 1802, before his college career closed, he was offered and he accepted the chair of Moral Philosophy in the University of Wilna, in West Russia, in which also, I believe, two other distinguished men were invited to become Professors—Thomas Campbell, the author of "The Pleasures of Hope," and Sir David Brewster, who has now succeeded Dr Lee in the office of Principal in our own University. It is but fair to say that these invitations were made through the medium of the late David Earl of Buchan, who, with some peculiarities of character, was a man of talent and taste, and inspired by a sincere zeal for the advancement of literature and science. Dr Lee prepared himself for fulfilling the duties of this appointment by writing out in Latin a portion of the lectures which he proposed to deliver at Wilna, but the arrangement was broken off by political events which interfered with its completion.

For some time previous to the end of 1805, Dr Lee had been on intimate terms with Dr Carlyle, well known as an eminent clergyman of the Church of Scotland, and then minister of Inveresk, near Edinburgh. He lived a good deal with Dr Carlyle, both at Inveresk Manse and in the Doctor's town residence; and as Dr Carlyle was then about eighty years of age, and still intimate with those of his own contemporaries, who were alive, such as John Home and Adam Fergusson, who belonged, like himself, to a by-gone age, and who had witnessed many remarkable events and social changes, it cannot be doubted that Dr Lee must have derived from this acquaintance a great deal of traditional knowledge as to the civil and ecclesiastical history of Scotland in the eighteenth century, and his natural bias may have been confirmed towards that historical research, and that interest in personal character and anecdote, by which he was afterwards distinguished. Dr Carlyle, at his death in 1805, appointed Dr Lee one of his trustees, and committed specially to his care an autobiographical memoir, which cannot fail to be full of interest, and as to which, I may be permitted to express a hope, that it will ere long be communicated to the public.

Among other eminent clergymen who befriended Dr Lee in the outset of his career, special mention ought also to be made of Dr Finlayson, of whom he always spoke in terms of the warmest regard, and to whose memory he has dedicated one of the painted windows now put up in the Old Greyfriars' Church.

About the same early period, Dr Lee came to be for some time connected with the late Sir John Lowther Johnstone of Westerhall, in the capacity of tutor or guardian, and was thus brought into contact with several eminent public men, with whom Sir John was on familiar terms. I have heard that Sir John made to Dr Lee two offers, either of which, if accepted, would have materially altered his future course in life. One was, to bring him into Parliament for one of Sir John's burghs; the other, to procure him a commission in the Guards. These offers, if made, were certainly declined; but he retained his ward's friendship and respect, and, from his gratitude, derived, during life, a pension of L.100 a year, which Sir John settled on him.

After taking his medical degree, he seems to have entertained some idea of following medicine as a profession; and he has been heard to say, that at one time, when a young man, he had three medical appointments in his possession or power; one, as assistant surgeon to a regiment; another, as surgeon's mate on board a ship; and a third, as a surgeon in the East India Company's Service. Finally he rejected all thoughts of the medical profession, and fixed upon the Church as the field to which he should dedicate his life.

In 1807 Dr Lee became minister of a Scotch Chapel in London, and, in the same year, he was presented to the parish of Peebles. He continued there till 1812, when he became Professor of Church History in St Mary's College, St Andrews, where he remained till 1821. A portion of the lectures he then delivered, embracing the History of the Church of Scotland from the Reformation, is now announced for publication, and cannot fail to excite a lively and general interest.

In 1820, before quitting his chair at St Andrews, he was appointed Professor of Moral Philosophy in King's College, Aberdeen, where he lectured for one session, chiefly by a deputy, to whom he transmitted his lectures daily by post. He speedily resigned his chair at Aberdeen, and in 1821 was removed to the charge of the parish of Canongate, Edinburgh; and thereafter, he successively held the other charges of Lady Yester's Church, and the Old Church Parish, in this city.

In 1824 he was named one of the Royal Commissioners for visiting the Scotch Universities. In 1827 he became Principal Clerk of the General Assembly. In 1837 he was appointed Principal of the United College of St Andrews, but did not long retain the appointment. In 1838 he was offered, but declined, the appointment of Secretary to the Bible Board, then newly constituted.

In 1840 he was elected Principal, and in 1843 he was appointed Professor of Divinity, in the University of Edinburgh. Previously, during the session of 1827–28, he had taught gratuitously the Divinity class, and afterwards, during the session of 1851–52, he taught gratuitously, again, the Moral Philosophy class, and in

1853–54, the Church History class, in the College of Edinburgh, during vacancies in those chairs occasioned by the death or the illness of their Professors.

He held the appointments of Chaplain to the Queen, of Dean of the Chapel Royal, of Chaplain to the Royal Academy, and to the Convention of Royal Burghs, and he was at his death one of the Vice-Presidents of this Society.

I have ventured to say that he was one of the most learned men of his time, and in some departments of National and Church History, particularly in all that concerns the civil and ecclesiastical affairs, as well as the manners and habits of the people of Scotland, during the sixteenth and seventeenth centuries, his knowledge was most minute and accurate. He was also at home in the cognate subject of the History of the Puritans during the same period. We have lately witnessed in this city the exposure to sale of a portion of his library, consisting of upwards of 20,000 volumes, some of them of the most rare and curious description; and I believe that there was not one of his books with which he was not familiar, and of which he did not know, as well as it could be known, the authorship, the occasion, the object, and the import. The subject of Bibliography had been from his early years a favourite study; and his habits of assiduity and perseverance, as well as his capacious and retentive memory, enabled him to prosecute it with singular success. Nor was his intellectual power overlaid or paralysed by the immense mass of his acquired knowledge. His opinions on all subjects, and particularly on those to which he had directed his special attention, were clear and comprehensive; while, at the same time, they were marked by that candour and moderation, which I believe to be universally produced by the thorough and accurate study of any branch of knowledge or portion of history.

As in the case of many men of learning and talent, his published works are but an imperfect indication of his actual powers. Principal Lee, however, has left some things behind him, such as the "Memorial for the Bible Societies," and the "Pastoral Addresses" composed by him for the General Assembly, which show at once the force of his understanding, the variety and accuracy of his information, the rectitude of his feelings, and the purity of his taste. His stores of learning also were always at the service of those who wished to make use of them, and his ready aid has been repeatedly acknowledged as having given additional value to some of the most important works of our time on ecclesiastical or antiquarian subjects. I would fain hope that, among his numerous papers, much may yet be found that deserves and demands publication.

Dr Lee's health had never been robust, and was probably injured in early life by habits of abstinence and excessive study. But it was wonderful with what energy and vigour he discharged his duties and followed out his favourite pursuits. He died on 2d May 1859, in the 80th year of his age, and in circumstances which had a melancholy connection with the death of a dear son just returning from India.

No man could be more universally regretted; he had not an enemy or an ill-wisher in the world. The numerous appointments which he successively and simultaneously held are a proof of the esteem and respect with which he was regarded by all; but those only who knew him well can speak to his amiable disposition, to his cheerful and genial habits, and to the charity and Christian kindness which he extended to all men of worth and merit, of whatever opinions or whatever persuasion. An account of Dr Lee, indeed, would be very inadequate if it did not prominently bring forward what I have thus alluded to—his highly amiable and affectionate character. In early life he earned on all sides the love as well as the respect of those who knew him. In his ministerial charge at Peebles, he was long remembered for his quiet and unostentatious, but most faithful discharge of his pastoral duties, for his ready and hearty sympathy with all who needed it, for his consolatory tenderness to the sick, and his great liberality to the poor. Nor were these qualities of the heart extinguished or impaired by the long life of labour and study which he afterwards led; on the contrary, they continued to the end. He was ever ready to relax into a playful cheerfulness and pleasantry in society; while his attention to such of his friends as from sorrow or suffering had more serious claims upon him was unremitting and invaluable.

In consequence, perhaps, of some defect of manner, Dr Lee was not sought after as an attractive preacher. But his sermons were excellent, both in matter and in style, and some of his earlier ones, when read in manuscript, had reached and obtained the approbation of Royalty itself. In other respects he was all that a minister of the gospel ought to be. Orthodox in doctrine, evangelical in sentiment, and blameless in conduct, he had a frankness and freedom from professional pedantry or clerical rigour which are rarely met with in men of his learning and condition. We shall not soon see his like again, if we ever do so in our day. Piety, zeal, eloquence, and assiduity will not be wanting to the Church; but the combination of these with the learning, the wide range of information and sympathy, and the knowledge of the world which he possessed, will not readily be found again.

The next name I have to record among those who have been taken from us, is that of William Pulteney Alison, who was also, at his death, a Vice-President of the Society. Dr Alison was the eldest son of a most amiable and excellent man, the Rev. Archibald Alison, long an Episcopal minister in this city, well known for his elegant published sermons, and for his Essay on Taste, in which he explained with much success his views of the influence of association in producing or heightening the sense of beauty, a theory which, within moderate limits, is founded on truth, but which has been brought into discredit by the extravagant length to which it was unfortunately carried in Lord Jeffrey's dissertations on the same subject.

Dr Alison in early life had the advantage of the best society which Edinburgh could boast of, and of which his father was a cherished and distinguished ornament. His education and connections led him to bestow much attention upon the subject of mental philosophy, which he cultivated with great success. Bnt he ultimately adopted medicine as his profession, to which he was probably drawn by the example and influence of his distinguished relative the late Dr James Gregory, and in which he was destined to find an appropriate career for his talents, acquirements, and virtues.

It would be idle in me to detail or dilate upon the particulars of his professional life, which was in all respects eminently successful, and in the course of which he came to hold a high place both as a teacher of medical science and as a practising physician. The notice of him which has lately appeared in the "Medical Journal" is so full and complete as to leave nothing to be desired in this respect; and if I were to attempt to abridge it, I should only weaken its effect, and probably fall into errors from which no unprofessional man can easily keep free. Neither can it be necessary to inform any one here present of the valuable contributions which Dr Alison made to the theory of medicine, or of the great skill, the indefatigable patience, and the unfailing benevolence by which, as a physician, he was uniformly distinguished. His published works are generally regarded as entitling him to a high place as an expounder of the philosophy of medicine, and his powers as an oral teacher were peculiarly efficient, and exercised a marked influence on the progress of medical science. The time, the strength, and the resources which he bestowed upon the sick poor were almost incredible, and such as no one could have given who to vigour of bodily frame had not added the impulse of the warmest benevolence and the highest principle. As a practical philanthropist, his name deserves to be placed not far behind that of Howard himself.

It would be a serious omission in any notice of this excellent man if his views and exertions, with reference to the Poor Laws of the country, were not in some degree commemorated. Two theories upon that subject, diametrically opposed to each other, were at one time advocated by two distinguished men in Scotland—Dr Chalmers and Dr Alison. Chalmers, misled, I think, by the enthusiasm of his own genius, and overlooking the peculiar powers which he himself possessed, conceived the romantic idea, that a compulsory or legal provision for the poor might be altogether dispensed with. He maintained, that even the great towns, if they were duly subdivided and furnished with a certain amount of religious machinery and superintendence, might be so purified and elevated in the scale of moral and physical wellbeing, that any pauperism which they might still produce could easily be relieved by the voluntary bounty of Christian benevolence. For this purpose he made the rather startling demand, that at least twenty new parishes and churches should be established in Glasgow. He was gratified to the extent of having one new church erected and assigned to him for the trial of his great experiment; and it is possible that by his own unwearied diligence and unrivalled influence, together with the auxiliary exertions of another most remarkable man, Edward Irving, who was given him as his assistant, the pauperism of his district may have been kept within manageable bounds, and sufficiently relieved by the spontaneous offerings of the wealthier parishioners. But it was obviously impossible that any such system could be established over the whole country; and even if such machinery had been provided, nothing short of a miracle could have supplied men like Chalmers and Irving in every district to carry out the plan. At the commencement of the attempt, doubts were raised by judicious thinkers as to its probable success; and subsequent reflection and experience soon converted those doubts into certainties, and produced a general conviction that the scheme was Utopian.

The views of Dr Alison on this important subject were essentially different. Indulging in no chimerical anticipations, better suited to a prophetical millennium than to the everyday state of actual things, he looked earnestly to the evils that were immediately operating or impending, and sought anxiously to remedy or avert them. He maintained that a compulsory contribution for the poor was indispensable. It was the only way of interesting the selfish portion of the rich in the welfare of their poorer brethren, by inducing them to take measures for diminishing pauperism, so as to save themselves from taxation. He contended that the relief of destitution could not be safely left to the precarious care of voluntary charity, but should at all hazards be provided for so as to keep up the general tone of society, and save it from moral and physical evils of the first magnitude. Destitution, he conceived, when without regular relief, tended to lower the standard of subsistence among the poor to an alarming degree, and to make them forget that there was any better state of things which it was worth their while as Christians, or as human beings, to aspire to. Destitution, he further asserted, and his assertion seemed to be proved by his medical experience, was one of the most fertile sources of disease, and particularly of disease of an epidemic character. It was at once, particularly in great towns, a predisposing cause to every form of pestilence; and by depressing vitality, it interposed the greatest obstacles to a cure. He thus endeavoured to demonstrate that the administration of adequate relief to paupers was indispensable for the public good, and a necessary measure of sanitary precaution.

These principles were, over a series of years, reiterated by Dr Alison, and pressed upon the public attention with all the fervour of deep conviction and ardent benevolence; and they were seconded within our own locality by the occurrence of alarming epidemics, which could not fail to rivet the public attention on the subject. If it is not presumptuous to say so, we seem to have reason to infer that the infectious nature of certain diseases is designed by Providence to quicken our interest in our fellow-creatures, and to remind us that our own welfare depends, in a great degree, on the health and happiness of our neighbours. As a conflagration in an adjoining house makes us tremble for our own safety, so the prevalence of fever or pestilence in the poorer classes of our own city excites in us the fearful anticipation that the mischief may soon extend to us or to our children. It was the object of Dr Alison to prove (and I think he succeeded in proving), that if we wish to avert epidemic and infectious diseases from our own doors, we must attend to the physical as well as moral condition of our fellow-citizens, and must establish a certain and sufficient provision for the poor.

The theoretical opinions of Dr Alison would probably have led him to exact a legal provision even for the able-bodied poor, but subject always to the condition that no one should receive support who was not ready to work. The practical question, however, scarcely extended to this point; and the result of the discussion finally was, that the views of Dr Alison obtained a triumph over those of an opposite tendency. The Poor Law Act of 1845 was passed; and a system of Poor Law relief was thereby established, which, I venture to say, deserves the highest commendation, and is fraught with signal benefits to the social condition of Scotland. The Scotch Poor Laws had always recognised a legal right to relief in the impotent poor; but, in practice, the frugality or parsimony of the national character had led to great abuses, by restricting the allowances made to paupers to such miserable pittances as were scarcely sufficient to sustain life; while the courts of law had but an imperfect jurisdiction to redress the evil. By the new law, a remedy is provided in the Board of Supervision, which practically has the power of seeing that adequate allowances are given to paupers by the local boards.

The Scotch Poor Laws had denied relief to the able-bodied poor; and it cannot be doubted that this question is one of a most delicate kind, as the right of the able-bodied poor to demand support might, if pushed to an extreme, lead to little less than a community of goods. The new Act still disallows any legal right in the able-bodied, but permits parochial boards to give them occasional relief, as a precautionary measure; and it is thought that this middle course effects a prudent compromise of the dispute.

The blessings, direct and indirect, which are likely to flow from this improved system of the Poor Laws, and from the increased attention thus given to the condition of the poor, may, in a great degree, be ascribed to Dr Alison's exertions; and his country owes to him, in this way, a debt of gratitude which even now it is difficult to estimate. The misery of the poor was alleviated, the tendencies to disease were diminished, the bonds of society were strengthened, and all were taught the important lesson that their own safety and happiness were indissolubly linked with those of other men.

It is curious to compare the early dawn and promise of Dr Alison's life with the character of its ultimate progress and development. The tastes and pursuits of his accomplished father were chiefly those that belonged to a man of elegant and pious contemplation. His own youthful aspirations are said to have tended towards a military life. The employments of his maturer years were certainly of a very different kind, though bearing still a strange moral analogy to these influences. He became engaged in a warfare, but it was with social misery and maladministration; and he carried it on in the pure and self-denying spirit of that great Exemplar who came into the world to heal our diseases and bear our infirmities, and who went about continually doing good. In the words of a distinguished friend, who knew him and loved him well, "it is not too much to say, that Scotland will mourn in him for one of the best of the Christian sons who have adorned her soil;—one who devoted himself, body and soul, to what he believed to be the good of his fellow-creatures, with a wisdom that looked beyond the present, with an energy that cast away all thought of self, with a Christian love that never failed."

The latter days of Dr Alison's life were clouded by the visitation of severe and distressing disease. With conscientious firmness, he resigned his professorial position, and retired into private life. To the last, however, he enjoyed intervals of serene and useful exemption from his sufferings; and it was only last year that he contributed to the Transactions of this Society an interesting notice of his cousin, the late Dr William Gregory. But the fatal ailment was making sure progress in his system, and terminated fatally on 23d September 1859, when he had attained his 70th year.

Another eminent and excellent member of this Society who has been taken from us is the late Lord Cathcart, for many years well known as Lord Greenock, his father having survived till the year 1843. A great part of Lord Cathcart's career belongs to professional or public life, and is fitter for the military or historical annals of the country than for the journals of a scientific society. His military services and distinctions, however, only made more conspicuous the devotion to science which he eminently showed.

Lord Murray, at one time a Vice-President of the Society, is another member whose loss we have to lament, in common with all who knew him, and in common, I may add, with very many whom he never knew.

We have also lost in this year the last of a most distinguished family of medical teachers, Dr Alexander Monro of Craiglockart, for many years Professor of Anatomy in the University of Edinburgh. He was in his 86th year when he died, having been born on the 25th of November 1773. He was the son of Alexander Monro the second, who again was the son of a distinguished father of the same name—all three being Professors in this University. The late Dr Monro was appointed assistant and successor to his father in 1798; and after his father's death, he occupied the chair with great reputation and success until 1845, when he resigned it, having, during that long period, numbered among his pupils many who became the most eminent physicians and surgeons of our day in both ends of the island.

Alexander James Adie, optician in Edinburgh, is another valued member of the Society whom we have lost. He was born at Edinburgh in 1775; and from the early death of his father, was thrown upon the care of a maternal uncle, Mr John Miller, optician in Edinburgh. Under his uncle's instructions, Mr Adie became an optician, and followed his profession with great diligence and assiduity.

His attention to business, with his skill as a mechanic, his quick inventive powers, and his sound judgment, led to his being much employed by all kinds of inventors, to give their schemes a practical form; and in this way he acquired great readiness and experience in the higher parts of his profession. His attention was at the same time directed at an early period to meteorological observations, with a view to which, and also with reference to the study of astronomy, he erected on his house in Merchant Court a small private observatory, long before any public establishment of the kind existed in Edinburgh.

While these pages were in preparation, we were deprived of another eminent and valuable member of the Society in Professor George Wilson, who has been suddenly carried off in the prime of life.

Dr Wilson was born in Edinburgh in the year 1818, and was thus, at his death, in the 41st year of his age. His parents were highly respectable, though not in such an elevated station as to diminish the credit due to his own exertions in attaining the position which he ultimately reached; but it deserves to be noticed, that he may be included in the number of distinguished men who have been in a great degree indebted for the development of their talents to the maternal character and influence. Dr Wilson's mother, a lady of remarkable intelligence, energy, and piety, is still living, to cherish the memory of his love to her, and of his many virtues and high reputation.

He was educated at the High School, where he always maintained a good place in his class. He entered the University of Edinburgh in or about the year 1834, and took his medical degree in 1839. In the interval, his attention came to be more specially directed to the chemical department of medical science, and he was successively engaged as chemical assistant to Dr Christison and to Professor Graham of London.

In 1840 he began to lecture in Edinburgh on chemistry in connection with the Extra-Academical Medical School. But at this time his health received a severe shock from the effects of excessive exertion during a pedestrian tour, which rendered amputation of the foot necessary, and ultimately led to a delicacy of constitution which settled upon his lungs.

He continued to teach as a private lecturer for upwards of fifteen years, and during that period secured the admiration, respect, and love of all who came in contact with him. His pursuit of knowledge was extensive and indefatigable, and his power of exposition was marked by the greatest clearness and animation, such as never failed to awaken in his hearers the strongest interest in the subject he was treating. He all along continued to cultivate a wide range of general literature, and his elegance of taste and reach of illustration were of much service in adding to the attraction of his prelections, as well as giving a great charm to his conversation, and to his literary productions. His published works and contributions to periodical literature are too well known to require detailed notice. Those which related to scientific subjects were distinguished by a minuteness of research and a precision of statement which give them a very great value, and which could hardly have been expected in one who was able at the same time to embellish them with so many beauties derived from his ample stores of imagination and fancy. His "Treatise on Colour Blindness" is a remarkable example of the exhaustive and practical manner in which he could treat such a subject; and his Lives of Scientific Men, while laudably compressed into a narrow compass, as compared with most modern biographies, are pregnant with valuable information and important results. He was in every way admirably qualified to diffuse among a wide circle of hearers and readers a strong interest in science as intimately connected with art and ordinary life.

In the spring of 1855 he was appointed Director of the Industrial Museum, a situation for which he was eminently suited; and in the autumn of the same year he was appointed to the Chair of Technology, then recently founded in the University of Edinburgh in connection with the Museum. It is needless to say in this meeting with what ability and success he discharged these duties. It was fondly hoped that in this congenial position, in the midst of friends and fellow-citizens who loved and appreciated him, and in the bosom of his own affectionate family, his constitution might gain strength, and that he might live to develop more fully, and perhaps in some new and original shape, the talents and genius of which he was possessed. But such was not the destiny appointed for him. He was sometimes, perhaps, too careless of consequences, where the call of supposed duty was heard, or where an opening of usefulness was afforded; and in the middle of much ill health, and many warnings of danger, he continued to exert himself in a manner that would have been more appropriate in one of robuster frame. But his pleasure lay in the exercise of his intellectual faculties, in the advancement of science, and in availing himself of every opportunity to do good or show kindness; and it is probable that the pious resignation with which he long contemplated his precarious condition, and the state of preparation which he constantly endeavoured to maintain against the approach of death, may have led him to fear that event less, and to despise precautions for his own safety which his friends would have wished him to adopt. I need not say that his talents and merits, as a man of science and literature, were equalled by the amiableness of his disposition, and by his moral and religious excellencies. He won, and he preserved, the friendship of some of the most eminent men of his time; and no one who came within the sphere of his influence could resist its attraction. The honours that he attained, and the success that attended him in life, were not considered by others to be more than he well deserved: but he himself was humble and unassuming; thankful for the mercies that he considered he had received, and, in the midst of much bodily suffering and distress, not merely patient and submissive, but cheerful and happy. His last illness was only a severer form of many previous attacks: but he had continued to labour to the last; and in particular his duties at the meeting of the British Association at Aberdeen, in the autumn of this year, were discharged by him under great debility, such as probably tended to unfit him for the severity of the winter that was at hand. The disease of the lungs having assumed a serious aspect, made rapid progress, and his death ensued on the 22d of November 1859. His end was calm and peaceful, such as became the pious, innocent, and useful life which he had led, and left his friends no cause to mourn, except for the loss which they themselves sustained.

19*th December* 1859.

1. Note on some Numerical Relations between the Specific Gravities of the Diamond, Graphite, and Charcoal Forms of Carbon and its Atomic Weight. By Dr Lyon Playfair, C.B., F.R.S.

Recent researches have shown that there is an intimate relation between the specific gravities and atomic weights or equivalents of solid and liquid bodies. This relation is not so simple as that which prevails in regard to the volumes and combining numbers of gaseous bodies, and yet it is sufficiently marked to indicate many important chemical analogies. The formula for eliciting these relations is—

$$\frac{E}{d} = V,$$

in which E is the equivalent, d the specific gravities, and V the atomic volume.

It is to be borne in mind, that the unities or starting-points for specific gravities and for atomic weights are essentially distinct. In the first case, the weights of the bodies are compared with the weight of an equal bulk of water; in the second instance, the combining numbers refer to a unit weight of hydrogen. Nevertheless, the relations observed between the specific gravities and the atomic weights are well marked in bodies of a like character.

It has always been considered interesting to examine these relations in regard to Carbon, which has three well-characterised allotropic forms. The atomic volumes obtained by the above formula show no satisfactory relations between the numbers obtained for each of the states in which the element presents itself.

Before we examine them in another way, it is desirable to obtain a mean specific gravity for the Diamond, Graphite, and Charcoal, as the recorded results of experiment show a considerable variation.

1. *Diamond.*

The specific gravity of this gem is generally stated in elementary works to range from 3·5 to 3·55; but these numbers do not represent the mean of recorded experiments, as will be seen by the following table:—

Diamond in Hunterian Museum, Glasgow,	3·53	Thomson.[1]
Specific gravity, as stated by Mohs, .	3·52	Mohs.[2]
Brazilian diamond, . . .	3·44	Brisson.[3]
Another variety of the same, . .	3·52	
Mean specific gravity of a "beautiful collection of diamonds," . . .	3·48	Lowry.[4]
"Star of the south," . . .	3·53	Dufrenoy & Halphin.[5]
Borneo diamond, . . .	3·49	Grailich.[6]
Do. do., compact, . .	3·41	Rivot.[7]
Do. do., do., . .	3·25	
Diamond used in Jacquelain's experiments,	3·33	Jacquelain.[8]
Specific gravity, as given by Henry, .	3·55	Henry.[9]
Well-crystallized Brazilian diamond, weighing 0·5761 gramme, in the Edinburgh Museum, . . .	3·48	Playfair.[10]
Mean sp. gr., .	3·461	

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[1] Thomson's Mineralogy, vol. i. p. 46.
[2] Mohs' Mineralogy, vol. ii. p. 306.
[3] Brisson, as quoted by Böttger, Specifiche Gewicht., p. 32.
[4] Lowry, as quoted in Thomson's Mineralogy, vol. i. p. 46.
[5] Dufrenoy, Compte Rendu, vol. xl. p. 3.
[6] Grailich, Bull. Geol. [2], vol. xiii. p. 542.
[7] Rivot, Ann. des Mines, vol. xiv. p. 423.
[8] Jacquelain, Ann. de Ch. et Thys, [2], vol. xx. p. 459.
[9] Henry's Mineralogy, vol. iv. p. 19.
[10] Experiment made for this paper.

If we reject the second Borneo diamond of Rivot, which has too low a specific gravity, we have a mean sp. gr. of 3·48, which is the same number as that found by Wilson Lowry for the mean specific gravity of "his beautiful collection of crystallized diamonds" (Thomson's Mineralogy, vol. i. p. 46).

It is to be expected that the experimental determination of the specific gravity of diamonds should be rather above than below the truth; for we are aware that they all leave a minute quantity of ash on burning, and that this ash, according to Petzhold, contains silica and iron.

2. *Graphite.*

This variety of carbon is often impure, being not unfrequently contaminated with upwards of five per cent. of earthy impurities. Recorded specific gravities upon such impure specimens are of no value for the mean result as regards pure graphite. The following determinations are all those which I can find upon specimens which have been chemically examined to establish their purity:—

Natural graphite, . .	2·27	Regnault.[1]
Do. . .	2·25	Schrader.[2]
Do. . .	2·32	
Graphite of iron furnaces, .	2·33	Karsten.[3]
Natural graphite, in fine crystalline plates,	2·14	Breikhaupt.[3]
Do. do., another variety,	2·22	
Do. do., do.,	2·23	Kengott.[4]
Natural graphite, . .	2·50	Pelouze and Fremy.[5]
Gas carbon graphite, . .	2·35	Graham.
Mean sp. gr., .	2·29	

It would have been interesting to have added to this list a determination of the specific gravity of Brodies' purified Ceylon graphite; but its minute division causes the air to adhere to it so tenaciously, that I have failed in getting any correct determinations of its density.

3. *Charcoal.*

There are comparatively few determinations of the specific gravity of pure charcoal. It is in fact not so easy to obtain this substance. A specimen of charcoal from pure sugar, repeatedly calcined, and treated with chlorine to remove the last traces of hydrogen, and again calcined, gave me the sp. gr. 1·80; but bubbles of air still adhered to it, although it was kept for several hours under a good air-pump. The following determinations are those recorded:—

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[1] Regnault, Ann. de Ch. et Thys., vol. lvi. p. 37.
[2] Schrader, Annals of Philosophy, vol. i. p. 299.
[3] As quoted in Böttger's Specifiche Gewicht.
[4] Kengott, Wien Akad. vol. xiii. p. 469.
[5] Traite de Chimie, vol. v. p. 518.

Pure lamp-black, . .	1·78	Baudrimont.[1]
Fibrous gas coke, . .	1·76	Colquhoun.[2]
Compact gas carbon, .	2·08	Baudrimont.[3]
Powdered coke (mean), .	1·80	Regnault.[4]
Charcoal from alcohol, .	2·10	Scholtz.[5]
Charcoal from sugar, .	1·80	Playfair.[6]
Pure charcoal, without pores,	1·84	Griffith.[5]
Mean sp. gr.,	1·88	

4. From the preceding data we take the mean specific gravity of the three varieties of carbon to be as follows:—

	Mean Sp. Gr.
Diamond, . . .	3·48 or 3·461
Graphite, . . .	2·29
Charcoal, . . .	1·88

5. We have now to consider whether these numbers stand in any simple relation to their atomic weight. The formula

$$\frac{E}{d} = V$$

gives the following atomic volumes, taking Ȼ = 12.

	Atomic Volumes.
Diamond, ...	3·44
Graphite, ...	5·24
Charcoal, ...	6·38

These numbers do not bear to each other any simple relation.

6. If we now take the atomic weight of carbon (Ȼ = 12), and then extract from it its square, cube, and fourth roots, numbers are obtained which bear a striking approximation to the mean specific gravity of the three forms of carbon:—

	Roots.	Sp. Gr.
1 -	$\sqrt[2]{12} = 3{\cdot}464$ -	Diamond, 3·48 or 3·46
2 -	$\sqrt[3]{12} = 2{\cdot}289$ -	Graphite, 2·29
3 -	$\sqrt[4]{12} = 1{\cdot}865$ -	Charcoal, 1·88

In other words, if we raise the specific gravity of diamond to its second power, that of graphite to its third power, and that of charcoal to its fourth power, we obtain numbers closely approaching in each case to 12, the atomic weight of carbon.

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[1] Baudrimont, Traite de Chimie, vol. i. p. 511.
[2] Colquhoun's Annals of Philosophy [2], vol. xii. p. 1.
[3] Baudrimont, Traite de Chimie, vol. i. p. 514.
[4] Regnault, Traite de Chimie, vol. i. p. 369.
[5] Böttger, Specifiche Gewicht.
[6] Experiment recorded above.

Diamond, . . .	$3{\cdot}48^2 = 12{\cdot}11$
Graphite, . . .	$2{\cdot}29^3 = 12{\cdot}00$
Charcoal, . . .	$1{\cdot}88^4 = 12{\cdot}49$

These approximations are remarkable, and the relations of the numbers are natural and simple. The differences between the mean experimental numbers and the corresponding roots of the atomic weight of carbon are not so great as the differences observed in the specific gravities of the same form of carbon.

7. It may be useful to condense into the form of a table the previous observations:—

Forms of Carbon.	Experiment.		Calculations.	
	Sp. Gr.	Powers.	$\cancel{\text{C}}$	Roots.
Diamond,......	$3{\cdot}46$ or $3{\cdot}48^2$	$= 12{\cdot}11$	$\sqrt[2]{12}$	$= 3{\cdot}464$
Graphite,......	$2{\cdot}29^3$	$= 12{\cdot}00$	$\sqrt[3]{12}$	$= 2{\cdot}289$
Charcoal, ...	$1{\cdot}88^4$	$= 12{\cdot}49$	$\sqrt[4]{12}$	$= 1{\cdot}865$

These relations appear to be so simple, that it is scarcely possible to conceive that they may not have been described before; but I have been unable to find such descriptions. The nearest approach to it which I know, is the fact that Mr Hawksley, the engineer, stated to me, many years since, that he had brought under the attention of the late Mr Cooper the relation which seemed to subsist between the specific gravities of silver and gold and their atomic weights, this being approximatively the square root of their atomic weights, or of multiples of these numbers. But I cannot find any record either of Mr Hawksley's or Mr Cooper's views on the subject.

8. We know two other bodies besides carbon which possess diamond, graphite, and amorphous forms—viz., Silicon and Boron. If the same relation were observed between the specific gravities and atomic weights of these bodies, it would go far to establish as a law what, in an isolated case, might be due to a remarkable combination of chances. Unfortunately, we know only the specific gravity of the diamond forms of these elements:—

Silicon diamond,	. .	2·49	Deville.
Do.,	mean on six specimens,	2·48	Playfair.

In quoting these results some explanation is necessary. In the original memoirs of Deville, it is left uncertain whether he examined the specific gravity of diamond or graphite silicon; and manuals of chemistry give it as the result due to the latter form; but from its coincidence with my own experiments on diamond silicon, it must unquestionably refer to that variety. Among the six specimens examined by myself, one preparation was in peculiarly fine crystals, and gave the sp. gr. 2·46; two out of the six specimens were prepared by Dr Matthiessien, and gave a mean sp. gr. of 2·47. The remainder were inferior samples, and probably contained zinc and other impurities.

Professor Miller has kindly examined for me the specific gravity of a good specimen of graphite silicon in his possession (not analyzed), and found it to be 2·337.

Deville gives as the specific gravity of boron diamond 2·68.

The crystalline form of the boron diamond is the same as that of the carbon diamond, and similar relations seem to exist between the specific gravity and atomic weights. The atomic weight of boron is 7·2, viewing its oxide as corresponding to carbonic acid in composition.

$$\sqrt[2]{7{\cdot}2}=2{\cdot}683. \qquad \text{sp. gr.}=2{\cdot}68.$$

But the same relation would not appear to hold for silicon, which does not affect the like tendency to crystallize in the same forms as carbon and boron, although the relations between the numbers in its case also are in the same direction, and not devoid of simplicity.

The atomic weight of silicon is 14·2.

$$\text{Si}\ \sqrt[3]{14{\cdot}2}=2{\cdot}42 \qquad \text{sp. gr. of diamond silicon,}\ =2{\cdot}46\ \text{to}\ 2{\cdot}48.$$
$$\text{Si}\ \sqrt[4]{28{\cdot}4}=2{\cdot}30 \qquad \text{sp. gr. of graphite silicon,}\ =2{\cdot}33.$$

The differences exhibited in this case from the similar forms of Carbon and Boron are not sufficiently marked to throw doubt upon the relations as being due to some unexplained law. As an arithmetical probability, indeed, the discordance lessens the value of the testimony in the previous cases. But our chemical knowledge of the manner in which Silicon doubles and quadruples itself in the silicates, to unite with the same quantity of base, gives support to the idea that its atomic weights may be different in the various forms of the separate element.

When we consider how much we multiply the errors of experiment in raising the observed specific gravities to the second, third, and fourth powers, it remains scarcely possible that the simple relations between them and the atomic weights, in the cases which I have pointed out, can be due to chance. I have purposely avoided any speculation as to the bearing which these relations may have on the molecular arrangement of the particles of the elements in their various forms, as I desire, in the first place, to submit the testimony on which the relations themselves are founded to the consideration of chemists.

But it may be fairly asked, whether any similar relations exist between the specific gravities and atomic weights of the remaining solid or liquid non-metallic elements. I take the following mean specific gravity for bromine, iodine, sulphur, and selenium, and omit the only two remaining elements—phosphorus and tellurium—from the list, because they do not appear to yield relations at all analogous to those under consideration:—

Bromine, sp. gr., ...	2·966	Balard.
	2·980	Lowig.
	2·990	„
Mean, . .	2·979	

Iodine.—There is only one recorded specific gravity of this element—viz., that by Gay Lussac. I have estimated the specific gravity of two fine specimens in my laboratory, and take the mean of these results:—

	4·948	Gay Lussac.
	5·030	Playfair.
Mean, . .	4·989	
Sulphur, sp. gr., . . .		=2·0
Selenium, . .	4·30	Berzelius.
	4·32	„
	4·31	„
Mean, . .	4·31	

Tabulating these results, and bringing into comparison with them the roots of the atomic weights, we have the following striking accordances:—

	Sp. gr.	Equivt.	Roots.
Boron, . .	2·68	$\sqrt[2]{7{\cdot}2}$	= 2·68
Silicon . .	2·46	$\sqrt[3]{14{\cdot}2}$	= 2·42
Bromine, . .	2·98	$\sqrt[4]{80{\cdot}0}$	= 2·99
Iodine, . .	4·99	$\sqrt[3]{127{\cdot}0}$	= 5·02
Sulphur, . .	2·00	$\sqrt[4]{16{\cdot}0}$	= 2·00
Selenium, . .	4·31	Se$\sqrt[3]{80{\cdot}0}$	= 4·31

2. Some Miscellaneous Observations on the Tadpole, and on the Albumen of the Newly-laid Egg. By John Davy, M.D., F.R.S. Lond. and Edin.

(This paper appears in the present number of the Journal.)

3. On Acupressure, a New Method of Arresting Hæmorrhage. By Professor Simpson.

3d January 1860.

The following Communications were read:—

1. Some Miscellaneous Observations on the Growth of Birds, their Specific Gravity, and on the Stomach of Fishes in Relation to Digestion. By John Davy, M.D., F.R.S. Lond. and Edin.

(This paper appears in the present number of the Journal.)

16*th January* 1860.

1. Suggested Explanation of Messrs Carrington and Hodgson's recently observed Solar Phenomenon. By Professor C. Piazzi Smyth.

The Royal Society of Edinburgh having been the arena wherein Professor W. Thomson first described his calculations and admirable extensions of Mr Waterston's meteoro-dynamic theory of solar light and heat, I beg leave to call the attention of the same learned Society to an apparent instance of confirmation which that theory appears to me to have received, by a phenomenon of very unique character, recently observed in an independent and most satisfactory manner, by either of two able scientific men—viz., Mr Carrington of the Observatory, Red Hill, and Mr Hodgson at Highgate.

The respective observations of these gentlemen are to be found in the monthly notices of the Royal Astronomical Society for November 1859, and seem quite sufficient to prove, after making all due allowance for the different instrumental methods employed in either case, and the peculiar nature of the subject observed, that on September 1, at about 11 h. 18 m. A.M., Greenwich time, two small telescopic bodies of light, in close proximity, and elongated in the direction of their motion, suddenly burst into view on the surface of the sun, not very far from its central portion, than which they were very much brighter. They moved side by side in arcs nearly parallel with the plane of the ecliptic; first for a time increasing in brightness, and then again gradually fading away, so as to be quite lost in about five minutes after their first appearance. Though apparently on the surface of the sun, yet that appearance was considered to arise from optical projection only, as they did not alter the shape of a group of large black spots, which lay directly in their paths. They must, nevertheless, have been exceedingly close to the surface; and on that supposition, the paths which they described during their period of visibility must, from their angular extent, have measured about 35,000 miles, giving a mean rate of 117 miles per second.

The first remark that we may make on the facts of observation, save that nothing so momentary has ever been witnessed before by astronomers, is, that 117 miles per second constitutes a velocity so exceedingly great, that we can only look to the gravitation influences of the sun for its efficient producing cause. Nevertheless, we are at the same time bound to acknowledge, that the full rate of orbital motion, for a body nearly in contact with the surface of the sun, is rather over 276 miles per second; and the rate of falling to the sun from infinite space, considerably more. Evidently, then, something prevented these bodies of September 1 from moving at their full rate, and produced a retardation in their orbit equal to 159 miles a second, to take Professor W. Thomson's form of the gravitation theory as the more probable.

What that retarding something was, it is not so much to our purpose now to inquire, as long as we can show that it is not altogether a baseless supposition, to assume the existence of any extensive material belonging to the sun, outside his visible, luminous surface. When we find our solar meteors of September 1 moving at a rate slower by 159 miles a second than they should do according to the laws of gravitation, the simplest assumption that we can make is, that there has been a mechanical retardation to that amount. Let this be granted, and then it necessarily follows from the dynamical theory of heat, that precisely in accordance with the disappearance of motion will be the appearance of heat.

According to Mr Waterston's form of the theory, and even the first expression of Professor W. Thomson's also, where the sun is fed by lumps of meteoric matter, there is some difficulty in explaining why the occurrence of luminous meteors in the sun is not frequently observed, if remarkably visible in one instance; while, according to the subsequent modification of the latter's view, the generality of meteors must be distilled away into impalpable clouds of finely-divided meteoric matter some time before they actually reach the sun; and neither gentleman had expected that an actual impact would ever be seen by mortal eye.

These objections, however, will be at once, to a great extent, relieved by the very fair assumption of as superior a mass to the September 1st meteor, over the generality of those which fall to the sun, in any and every manner, as men have already recorded of those which have actually fallen to, or have been seen very near, the earth; for while the majority (see the museums of Vienna, St Petersburg, and London) measure only a few inches, with occasional masses of 2 and 3 feet, there was one unusually well observed by many able spectators in Scotland, England, and France, on the 18th August 1783, which was estimated to be more than half a mile in length.

Let us inquire, then, how far the known meteoric mass of 18th August 1783 would suffice to produce the effects observed on September 1st, 1859, had it then fallen to the sun. Professor W. Thomson gives the amount of meteoric matter that would be required, according to Mr Waterston, to produce, by striking the sun, the average solar illumination, as 0·000060 lb. per square foot per second.

For the period of our phenomenon, or 5 minutes, this is ·018 lb. per square foot, and 501·811 lb. per square mile for the same time.

Now the meteor in question, said by Mr Cavalho to have had a diameter of 1070 yards, can hardly have contained less than 15,000,000,000 cubic feet; and if we take for its specific gravity a mean between what has been determined by many measures of earthy meteorites on one hand, and meteoric iron on the other (which comes extremely near the mean density of the earth), then the total weight must have been 5,250,000,000,000 lb. Whence it is evident that there was enough material in that one meteor, properly distributed, to keep a space 5,000,000 square miles of the sun's surface in a state of luminous ignition, twice as intense as that of the ordinary solar disc during all the time of observation; and therefore, by the transparency of flame, to have tripled the brightness of the parts passed over—a phenomenon which, from angular subtense, as well as intensity of light, would be abundantly visible to telescopic observation from our earth.

On the whole, then, it appears exceedingly probable that the solar phenomenon of September 1st was a meteor falling to the sun, and giving out the heat of its mechanical energy in accordance with the laws of that dynamical theory of thermotics, first and chiefly in this country brought before the Royal Society of Edinburgh by Professors W. Thomson and Macquorn Rankine. In which case, there is another example added, to several that might be extracted from the history of science, showing that hardly has a true theory been published to the world, before a confirmatory phenomenon, previously quite unexpected, is almost providentially witnessed; and in no case by less prejudiced or more able observers than the gentlemen upon whom we depend in the present instance.

2. On the Fallacy of the Present Mode of Estimating the Mean Temperature in England. By James Stark, M.D.

(This paper appears in the present number of this Journal.)

3. Description of the Plant which produces the Ordeal Bean of Calabar. By Professor Balfour.

After noticing the various plants used in Africa as ordeal poisons, the author gave an account of the introduction of the Calabar Ordeal Bean into Scotland, by the Rev. W. Waddell, and mentioned its peculiar poisonous qualities, as determined by Dr Christison. To Dr Hewan, and the Rev. Zerub Baillie, who are connected with the United Presbyterian Mission in Old Calabar, he was indebted for some observations on actual cases of poisoning in Africa. The Rev. W. C. Thomson, another missionary, was the first who procured flowering specimens of the plant. Some of these had been given to the author by Mr Baillie, and from them, along with the legume and seeds, the characters of the plant had been drawn up. The plant belongs to the natural order Leguminosæ, sub-order Papilionaceæ, and tribe Phaseoleæ, and appears to be a new genus to which the name of *Physostigma* (φυσάειν, to inflate) has been given, from the peculiar inflated appearance of the stigma. To the species the name of *venenosum* has been given, in allusion to its poisonous qualities. The genus is nearly allied to Phaseolus, from which it differs in the stigma, and in the long, grooved hilum of the seed. In the last character it approaches Mucuna.

Physostigma venenosum is a large twining plant, with a thick stem, and pinnately-trifoliolate leaves. The inflorescence is nodoso-racemose, the flowers being curved, and of a pale pink colour, the stamens 10, diadelphous, the style bearded at its upper part, and the stigma covered with a remarkable crescentic ventricular sac. The legume is 7 inches long, of a brown colour, containing two or three dark-brown seeds, with a long, deep hilum.

The paper was illustrated by drawings, executed by Dr Greville.

Botanical Society of Edinburgh.

Thursday, 8th December 1859.—Professor BALFOUR, V.P., in the Chair.

The following Communications were read :—

1. *On the Anæsthetic Effects of Chloroform, Ether, and Amylene, on Sensitive Plants.* By JOHN S. LIVINGSTON, Esq.

After explaining the method employed in performing the experiments, Mr Livingston proceeded to detail such of them as were of a typical character.

The anæsthetic influence it was found proceeded from leaf to leaf invariably in the descending order, and it very rarely happened that the leaf above the one acted on was at all disturbed. This effect was first observed by Professor Marcet of Geneva, and communicated by him in a paper to the Société de Physique. To whatever source this singular phenomenon might ultimately be traced, whether to a susceptibility of the descending sap for transmitting narcotic effects, or to the existence of some yet undiscovered organ which had that power, the fact was, at all events, beyond dispute.

To set aside any source of fallacy, and subject this fact to as severe a test as possible, the rootlets of the sensitive plant (*Mimosa pudica*), were carefully exposed, and doses of chloroform, ether, and amylene given, in order to see whether it was not possible in this way to induce a propagation of the influence upwards. In every case in which any effect was exhibited, it invariably proceeded downwards. When ether and amylene were employed, no effect, it is true, was produced; but in the case of chloroform, instead of the narcotic influence attacking first the leaf nearest the roots, as one would expect *à priori*, it passed by four of the leaves, and appearing first at the fifth, proceeded downwards till the first was reached.

De Candolle, in his "Physiologie Vegetale" (ii. p. 866), mentions some experiments made by him with sulphuric and nitric acids, on a sensitive plant, by which it was shown that these acids cause a folding of the leaves and a dropping of the petioles in an ascending order. These experiments had been repeated, and found substantially correct. A drop of sulphuric or nitric acid when placed on the lowermost petiole caused all the leaf-stalks to fall much below a right angle.

Of the three anæsthetic agents employed, amylene was found, on the whole, to act most powerfully on sensitive plants. With it the petioles always dropped down to more than a right angle with the stem, while with chloroform that was rarely the case. With both, the petioles dropped gradually and evenly, unlike what they did with ether, in which case the petioles literally fell down to a degree beyond that with amylene and chloroform. The following difference was, however observable :—In the two latter the falling of the leaf-stalks was always accompanied by folding of the pinnæ, while with ether that was not the case, showing that with it the effect was more local. When amylene was employed, the recovery from anæsthesia was very speedy compared with what it was when chloroform or ether was used. Moreover, the pinnæ, when touched with amylene, folded from apex to base with an increasing rapidity, till they sometimes became confused. This was very unlike the regular, and, if we may so call it, deliberate folding that took place with chloroform. With amylene, however, the pinnæ were always closely appressed, while with chloroform they were rarely so. With ether the effect did not show for some seconds, the number of which constantly varied.

2. *On the Primary Use of Ammonia in Vegetable Nutrition.* By Major John H. Hall.

The importance assigned to nitrogen in agricultural chemistry in the present day is a fact well known to all. It has come to be taken as the ultimate measure of the value of organic manures, and an analysis is not considered complete without specifying the quantitative amount of nitrogen which a manure contains. Observation of the avidity or capacity for ammonia which plants universally manifest has no doubt originally led to the conclusion that it contains something which must be highly beneficial in the economy of vegetable life. As ammonia is composed of hydrogen and nitrogen, the selection of the latter ingredient as the measure of value to the disregard of its other constituent, expresses a positive view or theory as to nitrogen being the all important element in ammonia which renders it so essential in the growth of plants. But I have never met with any satisfactory explanation of the grounds on which this estimate of the value and importance of nitrogen rests as an element of vegetable nutrition. I think that the true measure of the requirements by plants of any given substance should be found in the amount in which the substance enters into their composition. Now, an examination of the chemical constituents of vegetable substances shows that nitrogen enters very partially into them. Thus some of the most abundant of vegetable substances are entirely destitute of nitrogen. Cellulose, the structural basis of the roots, stems, leaves of plants, contains no nitrogen. Starch, gum, sugar, wax, oils, resins, some of the most abundant of vegetable products, are also destitute of it. Gluten is almost the only form or combination in which nitrogen occurs in plants, and it exists in them in small and variable quantities—in the seeds and fruits of some, and in the leaves of others; and it occurs for the most part in these plants and their products which constitute the food of man and animals, while the chemical constituents of plants fail to give evidence of their having any very great capacity for nitrogen. On the other hand, chemical experiments show the presence of hydrogen in every kind and form of vegetable matter. Its universality is on a par with that of carbon, and it is a remarkable circumstance, that it preserves a close relation with that substance, and generally follows it in the variations of its proportions in vegetable substances. These considerations led me to the conclusion, that the primary use of ammonia in the vegetable economy must be to supply hydrogen, to form in conjunction with carbon the hydro-carbonaceous material which forms the basis of all vegetable structures and productions, and that this alone can explain the reason why plants manifest such a universal avidity or capacity for ammonia. Not that I would question for a moment the concurrent use of ammonia in furnishing nitrogen to whatever extent the special requirements of particular plants may render necessary; but, looking to the very limited and partial extent in which it is found in vegetable productions, I apprehend it can never account for the universal capacity of plants for ammonia; and it seems to me, to say the least, a transposition and misuse of terms—the substitution of the minor and partial effect for the major and universal one—to regard ammonia only with reference to the constituent which has the least place in the vegetable economy, and to overlook that one which, equally with carbon, constitutes the universal pabulum of the vegetable creation. Major Hall then exhibited two plants of spinage, one of which had been watered simply with the common water of Edinburgh, and the other with a solution of carbonate of ammonia, and pointed out the great size which the latter plant had attained when compared with the former. The effects he endeavoured to trace mainly to the hydrogen in its combination with carbon.

Several members expressed doubts as to the correctness of Major Hall's conclusions, and pointed out the presence of nitrogen in the protoplasm or formative matter of plants as having been overlooked by him. It was stated to be the general belief of vegetable physiologists that no active cell-formation could go on without the presence of nitrogen, and that ammonia, whether in the atmosphere or in manures, was valuable in supplying this.

Dr Balfour exhibited a stem of *Astrapœa Wallichiana*, yielding a large quantity of mucilage. When the stem is cut and put into alcohol the exudation of this mucilage becomes very evident.

Dr Balfour also noticed that some stems of the Banana in the Botanic Garden, when allowed to dry after being cut down, showed a large quantity of white crystals on their surface. These had been analysed by Dr Simpson in the University laboratory, and had been found to consist of chloride of potassium.

Dr Maclagan, Berwick, sent roots of an elder tree, taken from a water-pipe, accompanied with the following note:—"The enclosed production was brought to me by the Superintendent of Works here. When moist it was much more bulky, but the radicles very brittle. It occupied and completely obstructed the main six-inch water-pipe, leading from the reservoir into the town of Berwick. The pipe is eight feet deep, and covered over with clay-puddle, through all which, and through some fissure at a joint, the small rootlet had penetrated. I asked what were the nearest plants, and found that two elders were suspected of being the culprits, and that they had been accordingly eradicated."

Thursday, 12th January 1860.—Professor Allman, President, in the Chair.

The following Communications were read:—

1. *Sketches of Caithness and its Botany, with a List of the Phanerogamous Plants and Ferns.* By Robert Brown.

This paper was the narrative of a botanical tour made in the autumn of 1859 in the County of Caithness, the Flora of which is by no means well known.

The author, after giving an outline of the physical geography of the county and sketches of its scenery, proceeded to describe its vegetation. The only indigenous trees of Caithness were stated to be *Populus tremula*, *Betula alba*, *Corylus Avellana*, and *Pyrus aucuparia*, and it was remarked that the specimens of these were comparatively stunted.

The county however appears at one time to have been covered with forests, numerous trees being annually dug up in the bogs. Trees, when planted, require to be protected from the sea breezes. The common crops of the fields (fenced into octagons, hexagons, heptagons, squares, and triangles, by what the geometry of the farmer accounts straight lines of upright flags) are excellent clover, grass, turnips (introduced within the last forty years), barley, bere or big (*Hordeum hexastichon*). Potatoes were only introduced about one hundred years ago; and though now extensively cultivated, were at first, for a number of years, limited to gentlemen's gardens. Before the introduction of potatoes, most of the ground now appropriated to them was devoted to the cultivation of cabbages, which constituted the principal vegetable food of the poorer classes. The common culinary vegetables grow well; but fruit trees, unless protected by a high wall with southern exposure, produce but indifferent fruit. Most of the crops are considerably later in coming to perfection than in the southern counties. For some time after Mr Brown arrived (July 31st), the hedges were pink with the wild roses, the haymakers were busy at their work for about three weeks, and the crops were not begun to be generally cut until the 1st of September. On Dunnett Links, the bent (*Ammophila arundinacea*) is instrumental in preventing the sand from blowing inward as formerly, spreading desolation for a great distance around. Many of the wild plants are applied to economic purposes—for instance, the pith of the common rush as wicks for the oil-lamps of the peasantry; heather for mats, ropes, &c. The number of the wild plants enumerated by Mr Brown was 419, exclusive of those introduced, &c., and about 29 well-marked varieties; but probably the number might be considerably increased. Mr Robert Dick of Thurso had been examining the Flora for many years, and to him, along with Mr C. W. Peach of Wick, the well known naturalist, the author had to tender his best thanks for the valuable assistance they had given him in drawing up this paper.

The following species are not noticed in Mr Watson's most valuable "Cybele Britannica" (vols. 1–4), as occurring in his "North Highland" province—(Ross and Cromarty, Sutherland and Caithness). Those regarding which further researches would be desirable are marked with a point of interrogation:—Ranunculus bulbosus, Papaver Rhœas, Plantago Coronopus (ballast), Cardamine impatiens (?), Viola odorata, Drosera intermedia, Tilia parvifolia, Hypericum humifusum, Prunus avium, Pyrus Malus (Westfield), Berberis vulgaris (hedges), Ribes alpinum, Saxifraga tridactylites, Ægopodium Podagraria, Valeriana dioica, Hieracium prenanthoides, Hieracium boreale, Antennaria dioica, Petasites vulgaris, Polemonium cæruleum? (outcast), Convolvulus sepium, Veronica polita, Myosotis palustris, Anagallis arvensis, Salsola Kali (ballast), Rumex sanguineus, Potamogeton plantagineus (?), Luzula Forsteri (?), Carex teretiuscula, Alopecurus fulvus, Alopecurus agrestis, Avena pubescens, Arrhenatherum avenaceum, Glyceria distans, Bromus sterilis, Hordeum murinum, Lastrea Fœnisecii, Equisetum umbrosum (common). The following may be also noticed as being species extending far north in Scotland, and not perhaps hitherto recorded in Caithness:—Ranunculus hederaceus, Arabis hirsuta, Barbarea vulgaris, Sisymbrium Sophia, Silene inflata, Lychnis vespertina, Hypericum quadrangulum, Erodium cicutarium, Geranium dissectum, Spartium scoparium, Prunus Padus, Geum urbanum, Alchemilla alpina, Pyrus Aria, Saxifraga stellaris, Sanicula europæa, Chærophyllum temulum, Myrrhis odorata, Sambucus Ebulus, Sherardia arvensis, Tragopogon pratensis (Reay Links), Hieracium vulgatum, Hieracium umbellatum, Carduus heterophyllus, Aster Tripolium (Mr Peach, Wick), Pyrethrum Parthenium, Scrophularia nodosa (?), Anchusa sempervirens, Pinguicula lusitanica, Lysimachia nemorum, Habenaria bifolia (Peach), Juncus balticus, Carex distans, C. limosa, C. pilulifera, Sesleria cærulea, Kœleria cristata, Festuca bromoides, Cystopteris fragilis, Asplenium Ruta-muraria, Isoetes lacustris.

Besides these there were noticed several other plants, which, though they have no effect in a phyto-geographical point of view, are yet interesting; such as—Draba incana, Vicia Cracca, Rosa canina (*b.* sarmentacea, Woods), Parnassia palustris (very common), Hieracium vulgatum (*e.* maculatum, Gm.), Cichorium Intybus (outcast), Antennaria dioica (*b.* norvegicum, Jacq.), Tussilago Farfara, Arctostaphylos alpina (Ben Shurraii), Menyanthes trifoliata, Nepeta Glechoma, Ajuga pyramidalis, Myosotis cæspitosa, Primula Scotica (very common), Euphorbia Helioscopia, Listera cordata, Potamogeton crispus, Sparganium simplex, Carex acuta, Hierochloe borealis, Asplenium Filix-fœmina, *b.* rhæticum (Roth.), *c.* molle (Hoffm.), Osmunda regalis, Lycopodium annotinum.

Many of those last have not been published as having been noticed in the county. So little has the botany been attended to by publishing naturalists, although the geology and zoology of the district have furnished valuable additions to the British Fossil and recent Fauna.

The author had not attended particularly to the cryptogamic botany of the county, although this department would reward research. Such lichens as Bæomyces roseus, Parmelia saxatilis, Parmelia physodes, Borrera tenella, Ramalina scopulorum, Lecanora subfusca and tartarea, Lecidea æruginosa, &c., are common.

2. *Notice of a Physiological Peculiarity in a specimen of* Tropæolum majus. By CHRISTOPHER DRESSER, Esq. Communicated by ALEXANDER DICKSON, Esq.

The author recorded a phenomenon which he had observed in a plant of *Tropæolum majus* growing in a damp part of one of his greenhouses. A pendant shoot of this plant had by accident become so much bruised and constricted, at a point about twelve inches from its extremity, as to prevent the transmission of sap from the root to the extremity of the branch, the terminal portions being connected with the rest of the plant merely by a fragment of withered bark and dried wood. This terminal portion, instead of presenting the very slight hairiness found in the ordinary state of the plant, had become extremely villous, the leaves being densely covered with white-looking hairs, so as to be quite velvety. The hairs were more densely congregated on the leaves than on the axis, and more so on the distal younger portions than on those nearer the seat of structure,—this latter circumstance, probably resulting from the hairs not being separated by the expansion of growth. The hairs on the petioles measured about $\frac{1}{18}$th or $\frac{1}{20}$th of an inch, being rather longer than those on the laminæ. The hairs were equally distributed over both surfaces of the leaf, and appeared to be a little longer on the veins.

The author alludes to the power possessed by hairs of absorbing dew, &c., and concludes that this portion of the plant had for weeks been nourished by the agency of these hairs; also that these organs were developed specially for the accomplishment of this end, since, in the ordinary condition of this plant, the hairs are extremely small and not numerous.

The author draws the inference that hairs are of little value as furnishing specific characters, since certain plants at least can and do protrude hairs under certain conditions.

3. *Notes on Californian Trees.* Part II. By ANDREW MURRAY, Esq.

(This Paper appears in the present No. of the Journal).

SCIENTIFIC INTELLIGENCE.

BOTANY.

Flora of Ceylon.—From the identity of position and climate, and the apparent similarity of soil between Ceylon and the southern extremity of the Indian peninsula, a corresponding agreement might be expected between their vegetable productions: and accordingly in its aspects and subdivisions Ceylon participates in those distinctive features which the monsoons have imparted respectively to the opposite shores of Hindustan. The western coast being exposed to the milder influence of the south-west wind, shows luxuriant vegetation, the result of its humid and temperate climate; whilst the eastern, like Coromandel, has a comparatively dry and arid aspect, produced by the hot winds which blow for half the year. The littoral vegetation of the seaborde exhibits little variation from that common throughout the Eastern Archipelago; but it wants the *Phœnix paludosa*, a dwarf date-palm, which literally covers the islands of the Sunderbunds at the delta of the Ganges. A dense growth of mangroves (*Rhizophora Candelaria, Kandelia Rheedei, Bruguiere gymnorhiza*) occupies the shore, beneath whose over-arching roots the ripple of the sea washes unseen over the muddy beach. Retiring from the strand, there are groups of *Sonneratia, Avicennia, Heritiera,* and *Pandanus;* the latter with a stem like a dwarf palm, round which the serrated leaves ascend in spiral convolutions till they terminate in a pendulous crown, from which drop the amber clusters of beautiful but uneatable fruit, with a close resemblance in shape and colour to that of the pine-apple, from which, and from the peculiar arrangement of the leaves, the plant has acquired its name of the Screw-pine. A little further inland, the sandy plains are covered by a thorny jungle, the plants of which are the same as those of the Carnatic, the climate being alike; and wherever man has encroached on the solitude, groves of coco-nut palms mark the vicinity of his habitations. Remote from the sea, the level country of the north has a flora almost identical with that of Coromandel; but the arid nature of the Ceylon soil, and its drier atmosphere, is attested by the greater proportion of euphorbias and fleshy shrubs, as well as by the wiry and stunted nature of the trees, their smaller leaves, and thorny stems and branches. Conspicuous amongst them are acacias of many kinds, *Cathartocarpus Fistula,* the wood apple (*Feronia elephantum*), and the mustard tree of Scripture (*Salvadora persica*), which extends from Ceylon to the Holy Land. The margosa (*Azadirachta indica*), the satin wood, the Ceylon oak, and the tamarind and ebony, are examples of the larger trees; and in the extreme north and west, the Palmyra palm takes the place of the coco-nut, and not only lines the shore, but fills the landscape on every side with its shady and prolific groves. Proceeding southward on the western coast, the acacias disappear, and the greater profusion of vegetation, the taller growth of the timber, and the darker tinge of the foliage, all attest the influence of the increased moisture both from the rivers and the rains. The brilliant *Ixoras, Erythrinas, Buteas, Jonesias,* *Hibiscus*, and a variety of flowering shrubs of similar beauty, enliven the forests with their splendour; and the seeds of the cinnamon, carried by the birds from the cultivated gardens near the coasts, have germinated in the sandy soil, and diversify the woods with the fresh verdure of its polished leaves and delicately-tinted shoots. . . . . Of the east side of the island the botany has never yet been examined by any scientific resident, but the productions of the hill country have been largely explored, and present features altogether distinct from those of the plains. For the first two or three thousand feet the dissimilarity is less perceptible to an unscientific eye, but as we ascend the difference becomes apparent in the larger size of the leaves, and the nearly uniform colour of the foliage, except where the scarlet shoots of the ironwood tree (*Mesua ferrea*) seem like flowers in their blood-red hue. Here the broad leaves of the wild plantains (*Musa textilis*) penetrate the soil among the broken rocks; and in most spots the graceful bamboo flourishes in groups, whose feathery foliage waves like the plumes of the ostrich. . . . . Still ascending, at an elevation of 6500 feet, as we approach the mountain plateau of Neura-ellia, the dimensions of the trees again diminish, the stems and branches are covered with orchideæ and mosses, and around them spring up herbaceous plants and balsams, with here and there broad expanses covered with *Acanthaceæ*, whose seeds are the favourite food of the jungle fowl, which are always in perfection during the ripening of the Nilloo (*Strobilanthes*). It is in these regions that the tree ferns (*Alsophila gigantea*) rise from the damp hollows, and carry their gracefully plumed heads sometimes to the height of twenty feet. At length in the loftiest range of the hills the Rhododendrons are discovered; no longer delicate bushes, as in Europe, but timber trees fifty to seventy feet in height, and of corresponding dimensions, every branch covered with a blaze of crimson flowers. In these forests are also to be met with some species of *Michelia*, the Indian representatives of the Magnolias of North America, several arboreous *Myrtaceæ* and *Ternstroemiaceæ*, the most common of which is the camellia-like *Gordonia zeylanica*. These and *Vaccinia*, *Gaultheria*, *Symploci*, *Goughia*, and *Gomphandra*, establish the affinity between the vegetation of this region and that of the Malabar ranges, the Khasia and Lower Himalaya.—*Ceylon;* by Sir James Emerson Tennent.

GEOLOGY.

Post-Pliocene Drift.—The Rev. W. Symonds, in a recent address to the Malvern Field Club, says—

"I have lately had the opportunity of examining carefully the mammalian relics and fossil shells obtained from the old river drifts of this neighbourhood, in the collections at Worcester, Jardine Hall, and Apperley Court, and, I may now add, of that of the Rev. William Parker of Little Comberton. The mammalian remains include those of the extinct elephant, that noble extinct bull the *Bos primigenius*, with the relics of the rhinoceros and hyena—quadrupeds no longer in existence on the continent of Europe, but which must, in former ages, have roamed on the ancient land, and whose skeletons were deposited in those old estuarine, lacustrine, and river margins which we have visited to-day. Among the fossil shells collected by the late Mr Hugh Strickland, from old lacustrine deposits at Cropthorne, Bricklehampton, and other localities near Pershore, are specimens of the *Cyrena consobrina*, which is found in the Nile, and ranges from Egypt to Cashmere and China, but no longer exists in Europe, with *Unio antiquior*, a remarkable form of river mussel which I believe is altogether extinct. When examining these shells in the cabinets of Mrs Hugh Strickland, I was astonished at the beautiful state of their preservation—the colour being in many instances preserved—thus rendering it impossible to arrive at any other conclusion than that the molluscs lived and died near the site where they are now found entombed. The accompanying shells are of the living species of Lymnea, Cyclas, Planorbis, &c., now found in the Avon and neighbouring brooks, and Mr Strickland has discovered in the *interior* of some of the fossil bones from the higher or *estuarine* gravels, some shells which I believe to be marine. The flint implements which have caused so much disquisition among geologists and naturalists were discovered in the north of France in undisturbed beds of gravel, sand, and clay, in drift, in fact, of much the same geological age as the old lake and river margins of the Avon and Severn. The level of the land, in that part of France, however, appears to have been more deranged by oscillating movements than has the water-level of our peaceful Worcestershire vales. Movements of upheaval and subsidence have occurred; and the stratified gravel, in which the supposed human implements are found, and which rests on the chalk (the basement rock of the country), is covered by a mass of newer unstratified drift. Nor is this all; for this stratified gravel, with the weapon-looking flints, and the bones of the elephant and the rhinoceros, is found in some instances at the height of 100 feet *above the present level* of the River Somme, which has worn down for itself a newer and a deeper bed since the mechanical-looking flints, and the bones of wild beasts long extinct in Europe, were buried together in the mud, and silt, and gravel of its *ancient* margins. I may here, then, reply to one or two questions which have been frequently put to me since my return from Aberdeen. *First*, Are we personally satisfied that the flints of the Somme Valley are implements fashioned by the hands of men? I reply, that the rudeness of very many of these implements might well cause the cautious investigator of truth to pause before he gave credence to their having been wrought by human beings; while, on the other hand, some specimens which were exhibited by Sir C. Lyell and Mr R. W. Mylne, appear to me to decide the question in favour of human agency. *Secondly*, Do the stratified gravels in which these relics occur afford indisputable evidence that man was co-existent with the extinct elephant, the rhinoceros, and other mammalia no longer living on the continent of Europe; may not these mammalian relics have been derived from older drifts washed from older beds? This is at present an unsettled point, and the most celebrated geologists of Europe are endeavouring to unravel the mystery; but I must candidly inform you that the most renowned tertiary geologists of England and France hold that it is impossible to avoid the conclusion, *if* these flints are *human implements*, that human beings lived at a far more remote period than was conjectured, when their creation was assigned to an epoch not more distant than some 6000 years, and that this opinion is arrived at from the evidence afforded by the *physical geology* of the district, by the *physical position* of the stratified gravels containing the supposed human implements, and independently of the question of the contemporaneity of man with the extinct animals."

MISCELLANEOUS.

Peculiar Sounds under Water.—On the occasion of another visit which I made to Batticaloa in September 1848, I made some inquiries relative to a story which had reached me of musical sounds said to be heard issuing from the bottom of the lake at several places, both above and below the ferry opposite the old Dutch fort, and which the natives suppose to proceed from some fish peculiar to the locality. The report was confirmed to me in all its particulars, and one of the spots whence the sounds proceed was pointed out between the pier and a rock which intersects the channel, 200 or 300 yards to the eastward. They were said to be heard at night, and most distinctly when the moon was nearest the full, and they were described as resembling the faint sweet notes of an Eolian harp. I sent for some of the fishermen, who said they were perfectly aware of the fact, and that their fathers had always known of the existence of the musical sounds heard, they said, at the spot alluded to, but only during the dry season, and they cease when the lake is swollen by the freshes after rain. They believed them to proceed from a shell, which is known by the Tamil name of (*Oorie coolooroo cradoo*), the "crying shell," a name in which the sound seems to have been adopted as an echo of the sense. I sent them in search of the shell, and they returned bringing me some living specimens of different shells, chiefly *littorina* and *cerithium* (*Cerithium palustre*). In the evening, when the moon had risen, I took a boat and accompanied the fishermen to the spot. We rowed about 200 yards north-east of the jetty by the fort gate; there was not a breath of wind nor a ripple except that caused by the dip of our oars; and on coming to the point mentioned, I distinctly heard the sounds in question. They came up from the water like the gentle thrills of a musical chord or the faint vibrations of a wine-glass when its rim is rubbed by a wet finger. It was not one sustained note, but a multitude of tiny sounds, each clear and distinct in itself—the sweetest treble mingling with the lowest bass. On applying the ear to the woodwork of the boat, the vibration was greatly increased in volume by conduction. The sound varied considerably at different points as we moved across the lake, as if the number of the animals from which they proceeded was greatest in particular spots; and occasionally we rowed out of hearing of them altogether, until, on returning to the original locality, the sounds were at once renewed. This fact seems to indicate that the cause of the sounds, whatever they may be, are stationary at several points, and this agrees with the statement of the natives, that they are produced by mollusca and not by fish. They came evidently and sensibly from the depth of the lake, and there was nothing in the surrounding circumstances to support the conjecture that they could be the reverberation of noises made by insects on the shore, conveyed along the surface of the water, for they were loudest and most distinct at those points where the nature of the land, and the intervention of the fort and its buildings, forbade the possibility of this kind of conduction. Sounds somewhat similar are heard under water at some places on the western coast of India, especially in the harbour of Bombay. At Caldera, in Chili, musical cadences are stated to issue from the sea near the landing place; they are described as rising and falling fully four notes, resembling the tones of harp strings, and mingling like those at Batticaloa, till they produce a musical discord of great delicacy and sweetness. The animals from which they proceed have not been identified at either place, and the mystery remains unsolved whether those at Batticaloa are given forth by fishes or by molluscs. Certain fishes are known to utter sounds when removed from the water, and some are capable of making noises when under it; but all the circumstances connected with the sounds which I heard at Batticaloa are unfavourable to the conjecture that they were produced by either. Organs of hearing have been clearly ascertained to exist, not only in fishes but in mollusca. In an oyster, the presence of an acoustic apparatus of the simplest possible construction has been established by the discoveries of Siebold; and from our knowledge of the reciprocal relations existing between the faculties of hearing and of producing sounds, the ascertained existence of the one might afford legitimate grounds for inferring the co-existence of the other in animals of the same class. Besides, it has been clearly established, that one at least of the gasteropoda is furnished with the power of producing sounds. Dr Grant, in 1826, communicated to the Edinburgh Philosophical Society the fact, that on placing some specimens of the *Tritonia arborescens* in a glass vessel filled with sea-water, his attention was attracted by a noise which he ascertained to proceed from these mollusca. It resembled the "clink" of a steel wire on the side of the jar, one stroke only being given at a time and repeated at short intervals. The affinity of structure between the *tritonia* and the mollusca inhabiting the shells brought to me at Batticaloa might justify the belief of the natives of Ceylon that the latter are the authors of the sounds I heard; and the description of those emitted by the former, as given by Dr Grant, so nearly resemble them, that I have always regretted my inability, on the occasion of my visits to Batticaloa, to investigate the subject more narrowly. At subsequent periods I have renewed my efforts, but without success, to obtain specimens or observations of the habits of the living mollusca. The only species afterwards sent to me were Cerithia, but no vigilance sufficed to catch the desired sounds, and I still hesitate to accept the dictum of the fishermen, as the same mollusc abounds in all the other brackish estuaries on the coast, and it would be singular, if true, that the phenomenon of its uttering a musical note should be confined to a single spot in the lagoon of Batticaloa."—*Tennent's Ceylon.*

Meteorological Society, &c.

Latitude 56° 34′ N. Longitude 2° 35′ W. Distance from the Sea, ⅜ths of a Mile.
Height of the Barometer above the Sea, 75 feet; height of the Thermometer from the ground, 11 feet, and of the Rain-Gauge, 2 feet.
The number of "Rainy Days" includes those on which snow or hail fell.

1859.	BAROMETER.		THERMOMETER.							Days Ther. below 32°.	Rain in Inches.	HYGROMETER.		Fair Days.	Rainy Days.	WINDS, AT 8½ A.M.									
	Corrected to 32°, and reduced to sea-level. 8½h. A.M.	8½h. P.M.	8½ A.M.	7¾ P.M.	Mean.	Mean Max.	Mean Min.	Mean.	Spring Water.			Mean Dew Point.*	Degree of Humidity (complete Sat. 1·000).			N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Calm.	Total.
January	29·86	29·85	36°·0	37°·2	36°·6	42°·2	32°·3	37°·2	47°·7	16	1·222	32°·2	0·876	19	12	1	—	—	—	2	10	11	3	4	31
February	29·71	29·71	38·8	37·6	38·2	44·4	34·2	39·3	47·2	10	1·339	34·0	·840	14	14	1	—	4	3	2	7	6	3	2	28
March	29·68	29·67	41·7	40·4	41·0	48·0	36·3	42·1	47·0	9	2·576	37·6	·858	19	12	—	1	1	—	3	5	16	4	1	31
April	29·75	29·75	41·7	40·5	41·1	48·2	34·3	41·2	47·3	15	2·149	35·8	·863	20	10	1	3	6	—	1	2	6	8	3	30
May	30·07	30·05	52·5	50·6	51·5	59·0	42·0	50·5	47·0	1	0·285	43·5	·735	28	3	3	4	6	12	3	—	1	—	2	31
June	29·94	29·93	56·8	55·2	56·0	63·8	47·6	55·6	48·0	—	2·702	49·3	·772	20	10	2	5	3	3	3	3	6	2	3	30
July	30·03	30·02	61·6	59·0	60·3	67·2	52·6	59·9	49·2	—	2·433	55·6	·820	22	9	—	5	2	2	7	5	4	1	5	31
August	29·82	29·82	58·9	57·3	58·1	66·3	49·5	57·9	50·2	—	1·416	52·2	·796	18	13	2	—	1	2	6	6	7	2	5	31
September	29·68	29·69	51·9	51·1	51·5	60·3	44·1	52·2	50·5	—	1·728	46·0	·816	17	13	1	1	1	2	4	4	9	4	4	30
October	29·62	29·60	43·5	45·4	44·5	49·8	39·2	44·5	50·4	9	4·095	39·6	·876	20	11	2	2	4	4	1	—	7	5	6	31
November	29·78	29·81	37·2	36·9	37·1	42·7	32·6	37·6	49·5	18	3·224	33·3	·862	19	11	2	—	1	4	2	2	6	8	5	30
December	29·58	29·60	32·2	33·1	32·6	36·4	28·7	32·5	48·0	25	2·209	29·7	·926	14	17	1	2	1	3	2	4	10	7	1	31
Mean	29·79	29·79	46·0	45·4	45·7	52·3	39·4	45·8	48·5	103	25·378	40·7	0·836	230	135	16	23	30	35	36	48	89	47	41	365
Do. 1858	29·90	29·90	46·3	46·3	46·3	52·9	39 9	46·4	48·0	71	24·353	40·2	0·817	242	123	20	12	26	47	44	37	71	66	42	365

Barometer at 8½ A.M. was highest on 10th November, 30·69.† Wind W.
Do. do. was lowest on 1st November, 28·44. Wind S.E.
Do. at 8½ P.M. was highest on 7th January, 30·65. Wind N.W.
Do. do. was lowest on 5th November, 28·62. Wind calm.
Thermometer at 8½ A.M. was highest on 17th July, 68°. Wind S.W.
Do. do. was lowest on 19th December, 17°. Wind W.
Do. at 7¾ P.M. was highest on 15th and 16th July, 68°. Wind W.
Do do. was lowest on 18th December, 22°. Wind N.W.
Thermometer in night, highest, 11th and 16th July, 61°; lowest, 19th December, 15°.
Thermometer in day, highest, 8th July, 75°; lowest, 18th December, 25°.
Coldest day, 19th December, when average of Thermometer was 21° for day and night.
Hottest day, 8th July, when average of Thermometer was 65½° for day and night.
Coldest month of the year, December; hottest, July.
Wettest month of the year, October; driest, May.
Mean temperature of the year, 45·75 degrees.
Mean temperature of sixteen years, 45·756 degrees.

For twelve years the average daily temperature at 8½ A.M., at 7¾ P.M., and the mean of the daily extremes, are as follows, viz.:—

	8½ A.M.	7¾ P.M.	Mean of Extremes.
1848	45°·1	44°·4	45°·0
1849	45 ·1	44 ·4	44 ·7
1850	45 ·5	44 ·9	45 ·5
1851	45 ·7	45 ·4	46 ·3
1852	46 ·0	45 ·9	46 ·2
1853	45 ·4	44 ·9	45 ·6
1854	46 ·3	46 ·4	46 ·3
1855	44 ·2	43 ·9	44 ·3
1856	44 ·9	44 ·5	45 ·0
1857	48 ·2	47 ·9	48 ·1
1858	46 ·3	46 ·3	46 ·4
1859	46 ·0	45 ·4	45 ·8

The average temperature of the six months of chief vegetation, viz., those from April to September inclusive—for twelve years, is as follows:—

1848	51°·7	1850	51°·7	1852	53°·0	1854	53°·5	1856	50°·9	1858	53°·5
1849	52 ·2	1851	52 ·0	1853	53 ·4	1855	52 ·1	1857	54 ·7	1859	52 ·9

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* The dew-point thus found is obtained from observations of the Wet and Dry Bulb Thermometers, and deduced by Mr Glaisher's Hygrometrical Tables. The observations of the Wet and Dry Bulb Thermometers are made daily at 8½ A.M. The times of observation of the instruments stated in the Tables are Greenwich mean times.

† The highest and lowest readings of the Barometer are reduced to sea-level, and otherwise corrected.

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INDEX.

Alison, Dr W. P., Biography of, 316
Alternation of Generation, 1
Ammonia, its Primary Use in Vegetable Nutrition, by Major John H. Hall, 334
Anæsthetic Agents, their Effect on Plants, 333
Arabic-speaking Population of the World, 139
Aral, Vegetation of Sea of, 163
Astrapœa Wallichiana, its Mucilage, 335
Aurora of 28th August and 2d September 1859, 90
Balfour, Professor J. H., on the Plant producing the Calabar Ordeal Bean, 332
Ballantyne's Christianity contrasted with Hindu Philosophy, Noticed, 103
Balloon experiment, 114
Birds, Growth of, 260
——— Specific Gravity of, 264
Bone Cave near Montrose, 308
Boring for Water, 311
Botanical Society of Edinburgh, Proceedings of, 141, 333
Bread Making, 125
Breaks for Railway Trains, by W. Fairbairn, 310
Brewster, Sir D., on a New Species of Double Refraction, 113
——— on Nineveh Glass, 121
British Association, Proceedings of, 108, 290
Butterflies, British, Distribution of, by H. T. Stainton, 306
Caithness, Botany of, by Robert Brown, 335
Caledonians, Ethnology and Hieroglyphics of, 139
Californian Trees, Notes on, 205
Carduella Scotica, 308
Ceylon, Flora of, 338
Chinese Astronomy, 120
Chloride of Potassium Crystals on the Banana Stem, 335
Claudet, A., on the Focus of Object-Glasses, 114
Coal-Mines of Borneo, by James Motley, 166
Compass in Iron Ships, 110
Cone-in-Cone Structure, 132
Cretinism and Goitre in the Cordillera, 29
Darwin's Origin of Species, Noticed, 280
Davy, Dr John, on the Tadpole; on the Albumen of the Newly-laid Egg; on the Growth of Birds, and their specific gravity; on the Stomach of Fishes, 252
Diamond, Graphite, and Charcoal Forms of Carbon, 323

Diaphragm for Double Achromatic Combinations, 115
Dinornis, Egg of, 164
Disguises of Nature, 66
Dynamical Theory of Gases, 122
Egg, Albumen of, 257
Electrical Frequency, 118
Electric Cable, 112
Electricity, Atmospheric, 108
——— transmitted through Water, 113
Elephant Remains at Ilford, 136
Ferns, Vegetative Axis of, by Dr Ogilvie, 310
Fish, Stomach of, in relation to Digestion, 266
Flints of Amiens, 130
Flora of Aberdeenshire, by Dr Dickie, 307
——— Ceylon, 338
Form and Colour in Plants and Animals, 66
Fuel, Use and Economy of, 45, 192
Galago, Supplementary Remarks on, 99
Gebel Haurân, and Eastern Desert of Syria, described Geographically and Geologically, 173
Geikie, A., on the Chronology of the Trap-Rocks of Scotland, 132
Geology of Aberdeen and North-East of Scotland, 126
Glaciers, Vestiges of Extinct. Part I., by Edward Hull, B.A., 31
Glass found at Nineveh, 121
Gneiss, Red Sandstone, and Quartzite, their Relation, 134
Goitre and Cretinism in the Cordillera, 29
Granite Quarries of Aberdeen, by A. Gibb, 311
Graptolithus, Remarks on, by James Hall, 167
Gymnotus electricus, 307
Hector's Exploration of British North America, 169
Heliometer, Improvement of, 120
Hogg, John, on Gebel Haurân and the Eastern Desert of Syria, 173
Hull on Extinct Glaciers, 31
Hybrids in Plants, 163
Japan, Botany of, by Professor Asa Gray, 159
——— Notes on, by Laurence Oliphant, 169
Jardine, Sir Wm., on the Progress of Zoology and Botany, 290
Jenkins, F., on Submarine Telegraphic Signalling, 111
Kirk, Dr, on the Country near Lake Shirwa in Africa, 151
Landscape in a specimen of Calcedony, 115
Lee, Principal, Biography of, 312
Lindsay, J. B., on Transmission of Electricity through Water, 113
Livingston, J. S., on the Anæsthetic Effects of Chloroform, Ether, and Amylene, on Sensitive Plants, 333
Livingstone's African Expedition, Noticed, 151
Lyell, Sir C., on the Progress of Geology, 129
Manuring Constituents of Crops, 125
Meteorological Register for 1859, kept at Arbroath by Alexander Brown, 343
Milk, Preservation of, 123
Milne, Alex. D., on Laws of Heat and Combustion, in Reference to the Use and Economy of Fuel, 45, 192

Moigno, Abbé, on the Phonautograph, 113
Mont Blanc, Thermometric Stations on, 117
Moon's Motion, 115
Morphology of certain Organs of Plants, by Christopher Dresser, 155
Morrhua vulgaris, and *M. punctata*, 306
Muir, John, D.C.L., on the Progress of Sanskrit Literature, 242
Murray, Andrew, Notes on Californian Trees, Part II., 205
——— on the Disguises of Nature, 66
——— on the Genus Galago, 99
——— on the Progress of Botanical Science, 141
Nicol, Professor James, on the Geology of Aberdeen and North-East of Scotland, 126
Object-Glasses, Focus of, 114
Ogilvie, George, M.D., on the Genetic Cycle in Organic Nature, 1
Organic Nature, Genetic Cycle in, 1
Owen, Professor, on Fossil and Recent Reptilia, 294
Oxides and Salts, Symmetrical Arrangement of, 124
Peruvian Crania, 25
Peruvian Gleanings, 25
Peruvian, Oriental Origin of, 26
Pheasants of Britain, 309
Phonautograph described, 113
Photograph of Fluorescent Substances, 125
Physostigma venenosum, the Calabar Ordeal Bean Plant, 332
Pinus insignis, 222
——— *Jeffreyi*, 224
——— *Murrayana*, 226
——— *radiata*, 222
Planetary Orbits, 122
Playfair, Dr Lyon, on a Symmetrical Arrangement of Oxides and Salts on a common type, 124
——— on some Numerical Relations between the Specific Gravities of the Diamond, Graphite, and Charcoal forms of Carbon, and its Atomic Weight, 323
Polyps, their Structure, 164
Post-Pliocene Drift, by Rev. W. Symonds, 339
Refraction, Double, New Species of, 113
Reptilia, Fossil and Recent, 294
Reptilian Remains near Elgin, by Professor Huxley, 134
Reviews and Notices of Books, 103, 271
Rogers, Professor William B., on the Aurora of 28th August and 2d September 1859, 90
Roots in Drains, 335
Royal Society of Edinburgh, Proceedings of, 312
Rudolph's Botanical Geography, Noticed, 271
Sanskrit Literature, Progress of, 242
Sequoia sempervirens, 221
Silurians of Lesmahagow, by Mr Page, 133
Skull found at Jerusalem, 164
Societies, Proceedings of, 108, 290
Sounds under Water in Ceylon, 340

Smith, Archibald, M.D., Peruvian Gleanings, 25
Smyth, Professor C. Piazzi, Explanation of Carrington and Hodgson's recently observed Solar Phenomenon, 330
Species of Plants, Distribution of, by Professor Asa Gray, 157
Stark, Dr, on the Incorrectness of the Present Mode of Estimating Mean Temperature, 228
Tadpole, 252
Telegraph, Submarine, 111
Temperature, Mode of Estimating, 228
Tertiary Fossils of India, by W. H. Baily, 135
Thomson, Wm., on Atmospheric Electricity, 108
——— on Discharge of a Coiled Electric Cable, 112
Towson, John T., an the Compass in Iron Ships, 110
Trap-Rocks of Scotland, Chronology of, 132
Tropæolum majus, Physiological Peculiarity in, by Christopher Dresser, 337
Voelcker, Professor, on the Essential Manuring Constituents of Cultivated Crops, 125
Volcanic Rocks in Italy, by Dr Daubeny, 133
Wellingtonia gigantea, 205
Willich's Popular Tables, Noticed, 107
Wilson, Dr Daniel, on a Fragmentary Skull found in an Ancient Quarry Cave at Jerusalem, 164
——— Professor George, Biography of, 321
Winters in Britain, 122
Wren, Sir Christopher, Cypher of, 119
Zoophytes of Caithness, by C. W. Peach, 307

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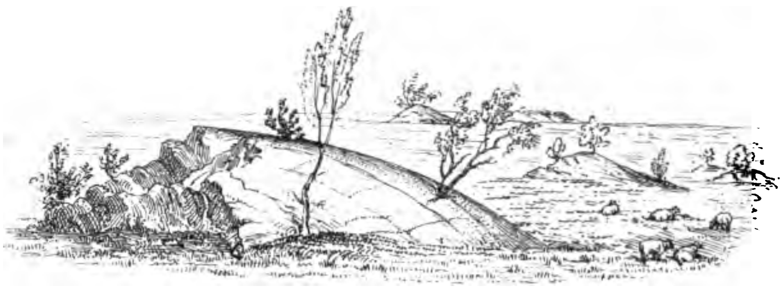
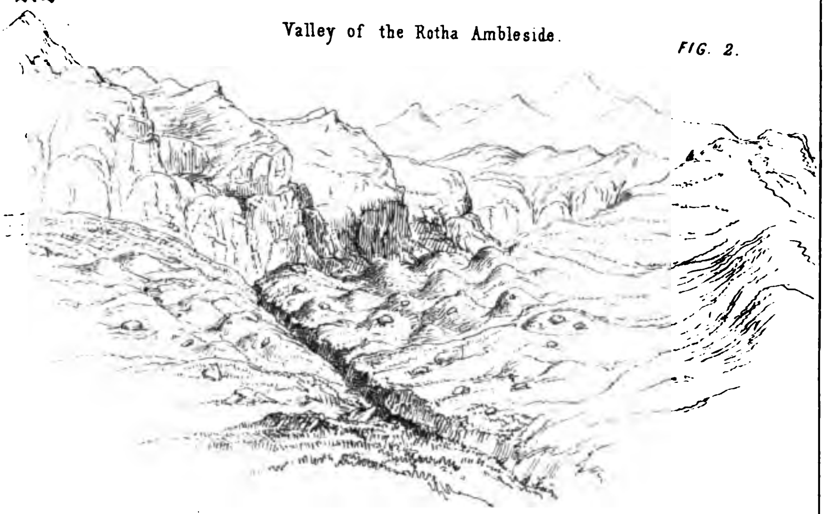


ROCHES MOUTONNÉES.

Valley of the Rotha Ambleside.

FIG. 2.



MORAINE AT THE HEAD OF THE STAKE PASS.

∽∽ Shoulder of Bowfell. ⟶ Skiddaw in the distance.

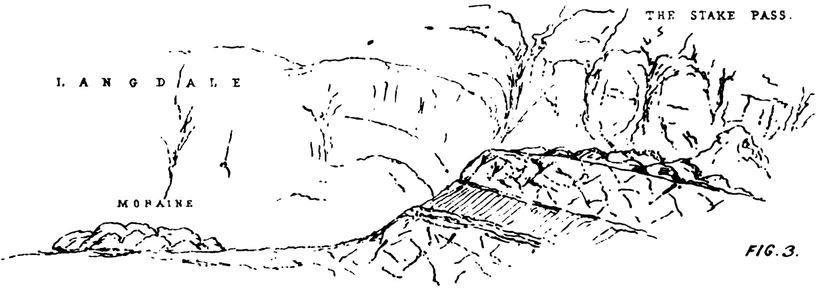
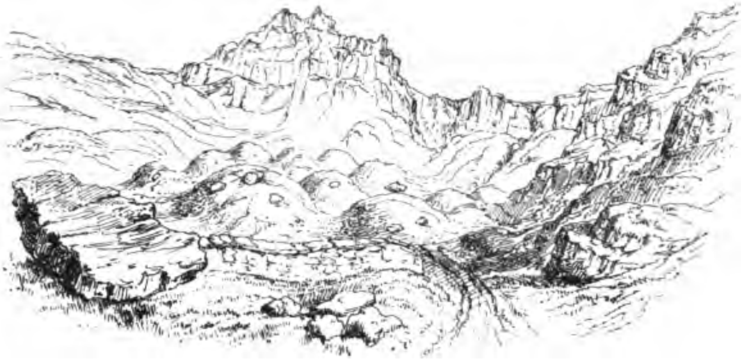


FIG. 3.

MORAINE OF LANGDALE & THE STAKE PASS.

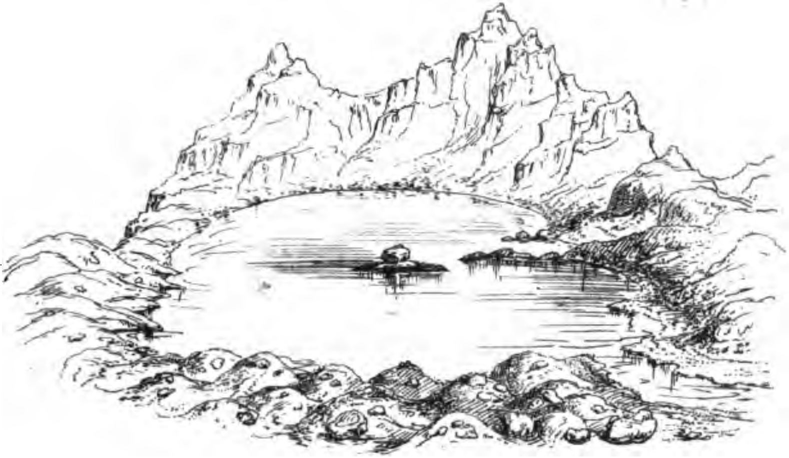
W. H. M'F.

FIG. 4.



MORAINE AT THE HEAD OF LITTLE LANGDALE.

FIG. 5.



STICKLE TARN.

Showing a Moraine in the foreground embanking the N. & E. Side of the Lake and a boulder perched on a boss of rock-rising from the Centre.

W. H. McFarlane, Edinʳ.

New Phil. Journal. New Series, Vol. XI. Pl. 3.

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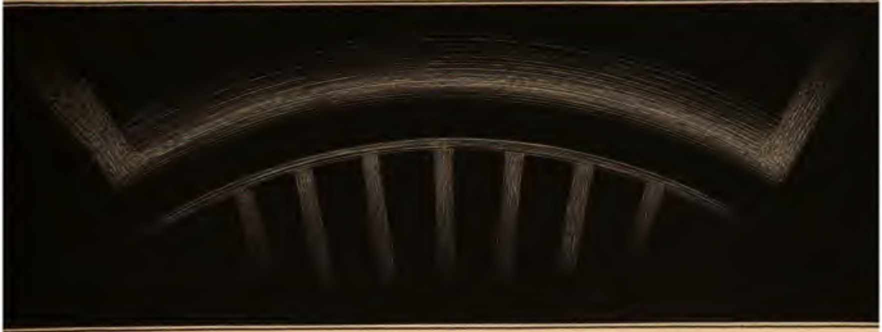

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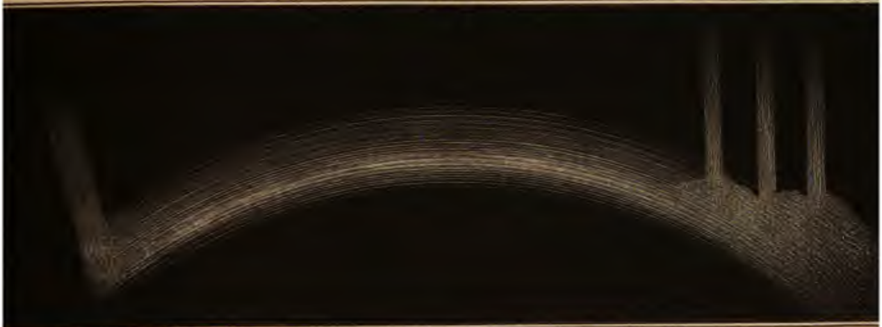


FIG. 3

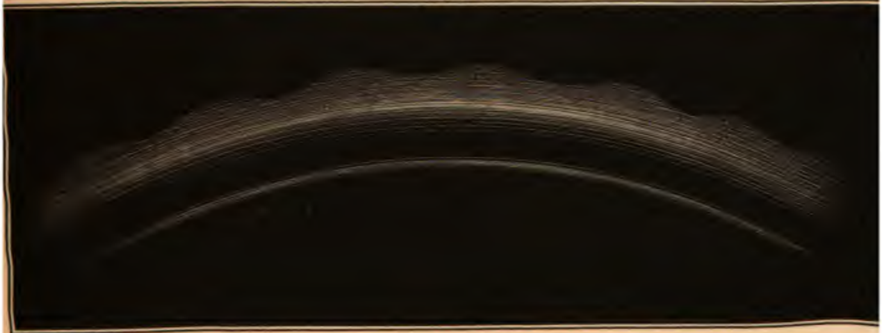
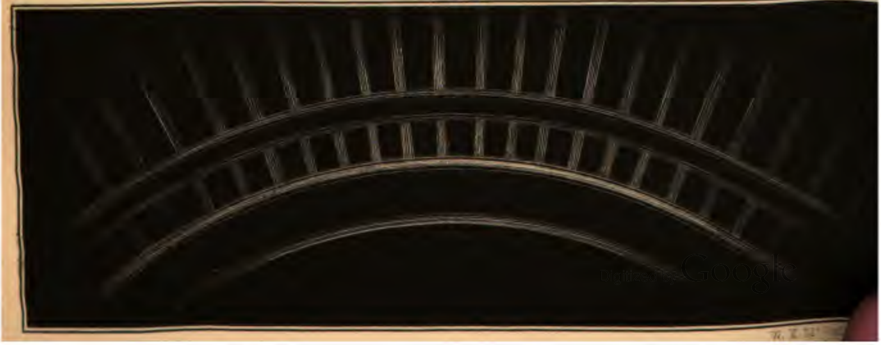
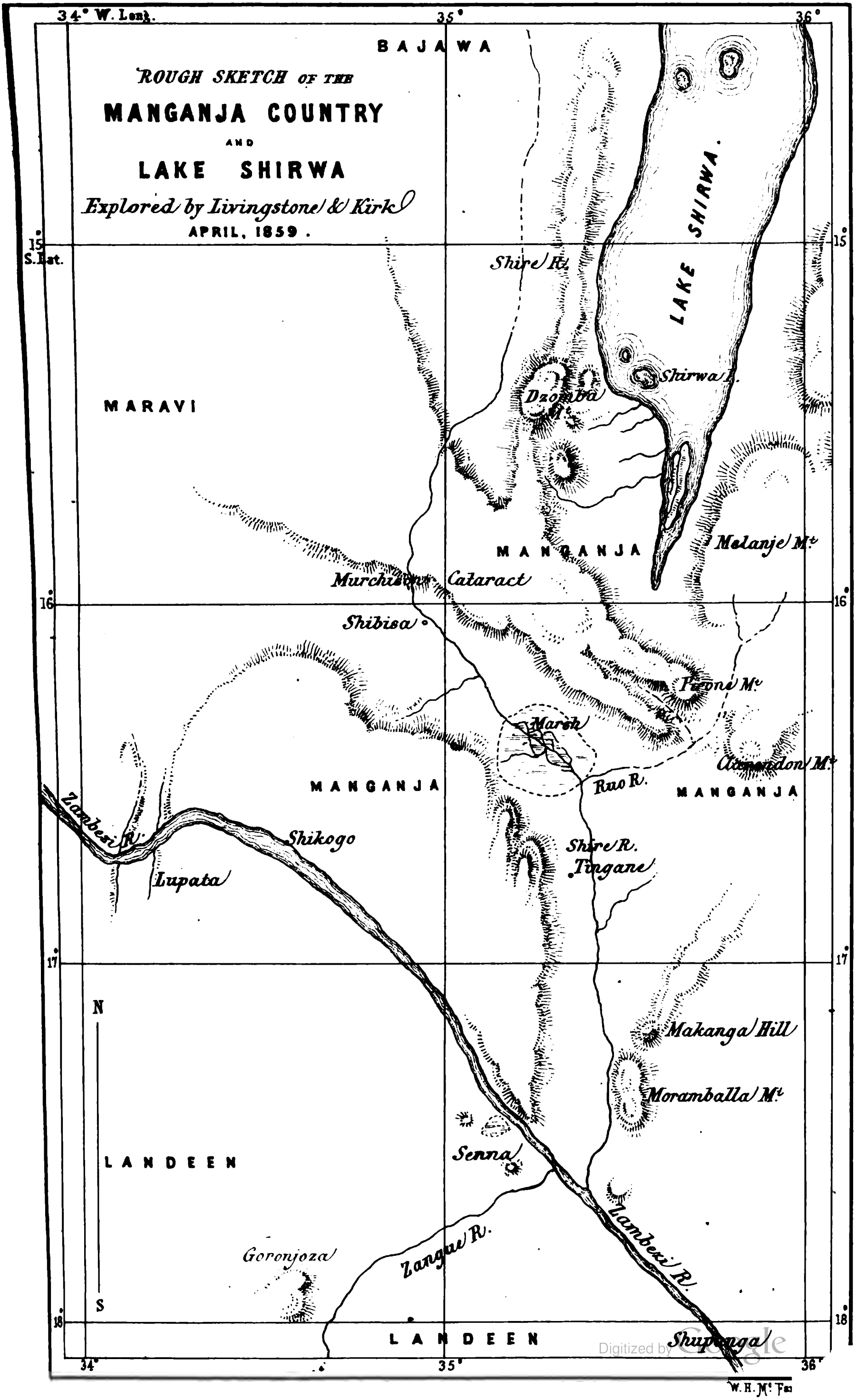


FIG. 4.





ROUGH SKETCH OF THE

MANGANJA COUNTRY

AND

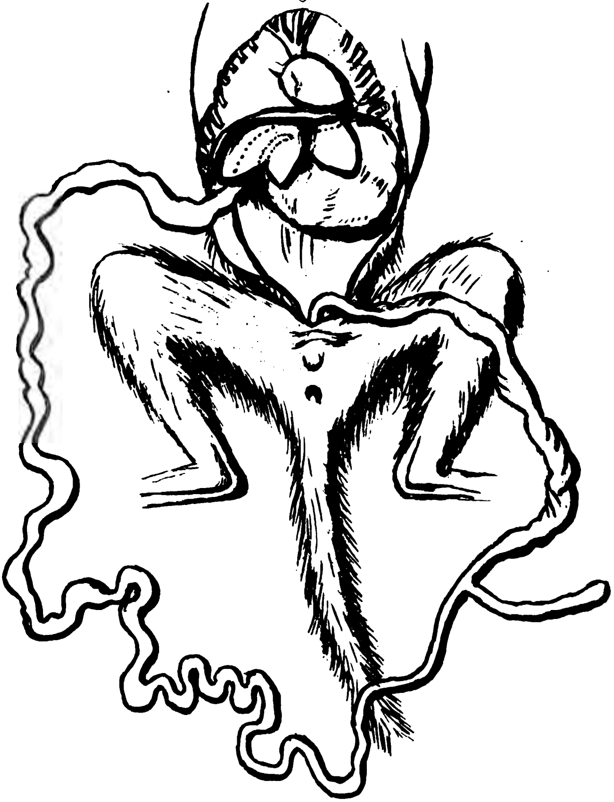
LAKE SHIRWA

Explored by Livingstone & Kirk

APRIL, 1859.

W. H. M^c. Far

Fig 1.



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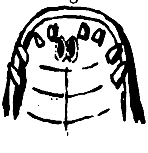

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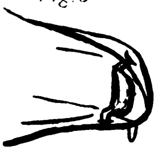

K. Greville, del.

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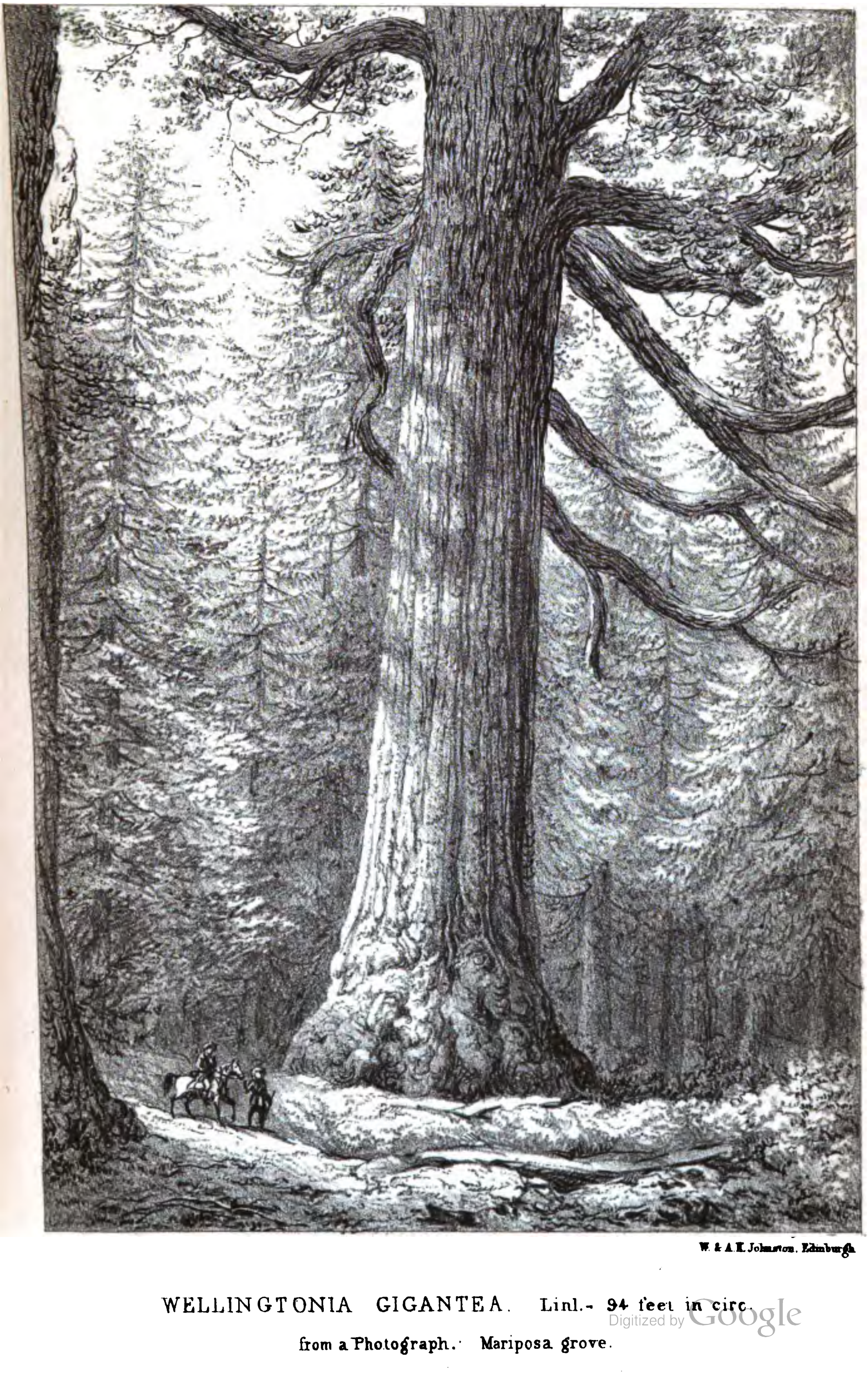

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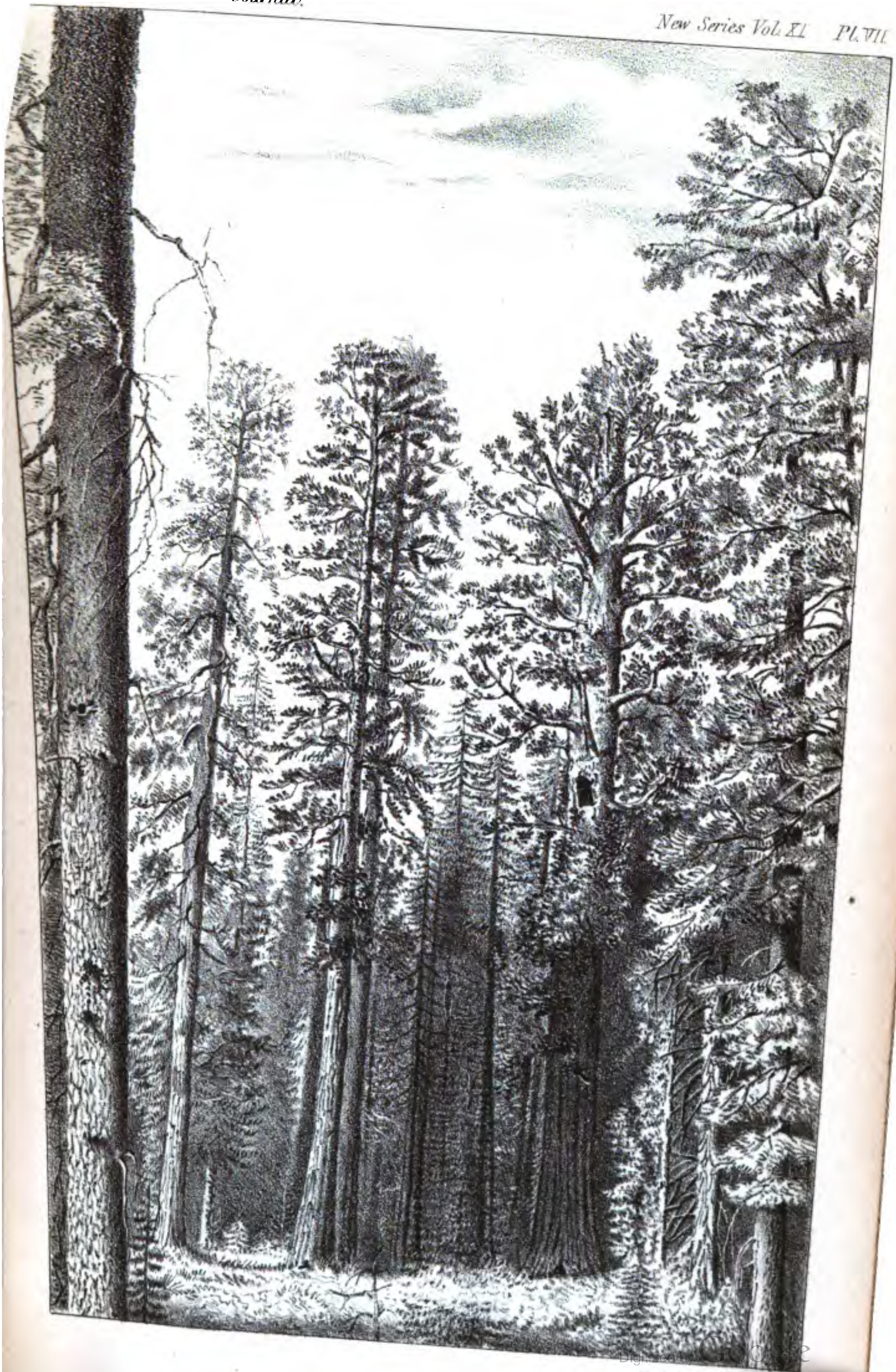
from a Photograph. Mariposa grove.



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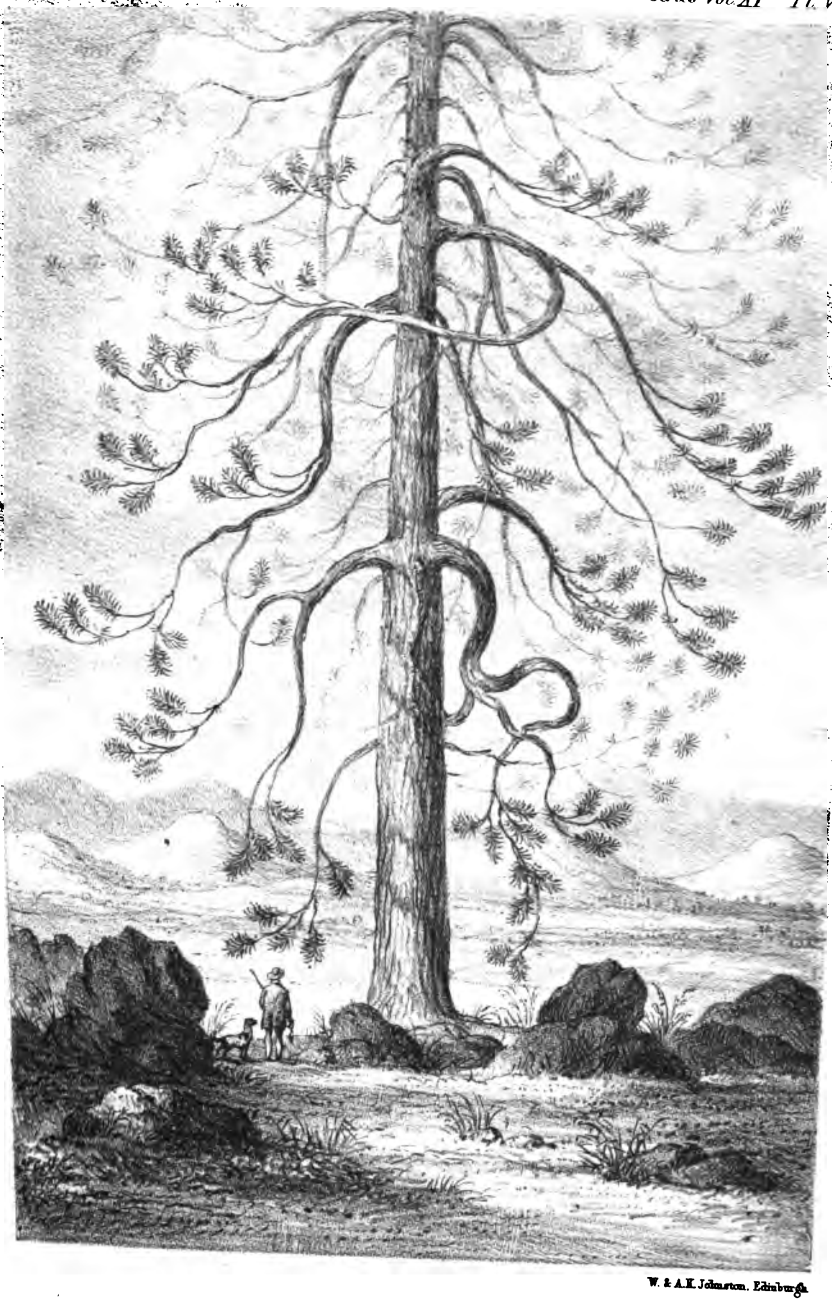
GROUP OF WELLINGTONIA GIGANTEA



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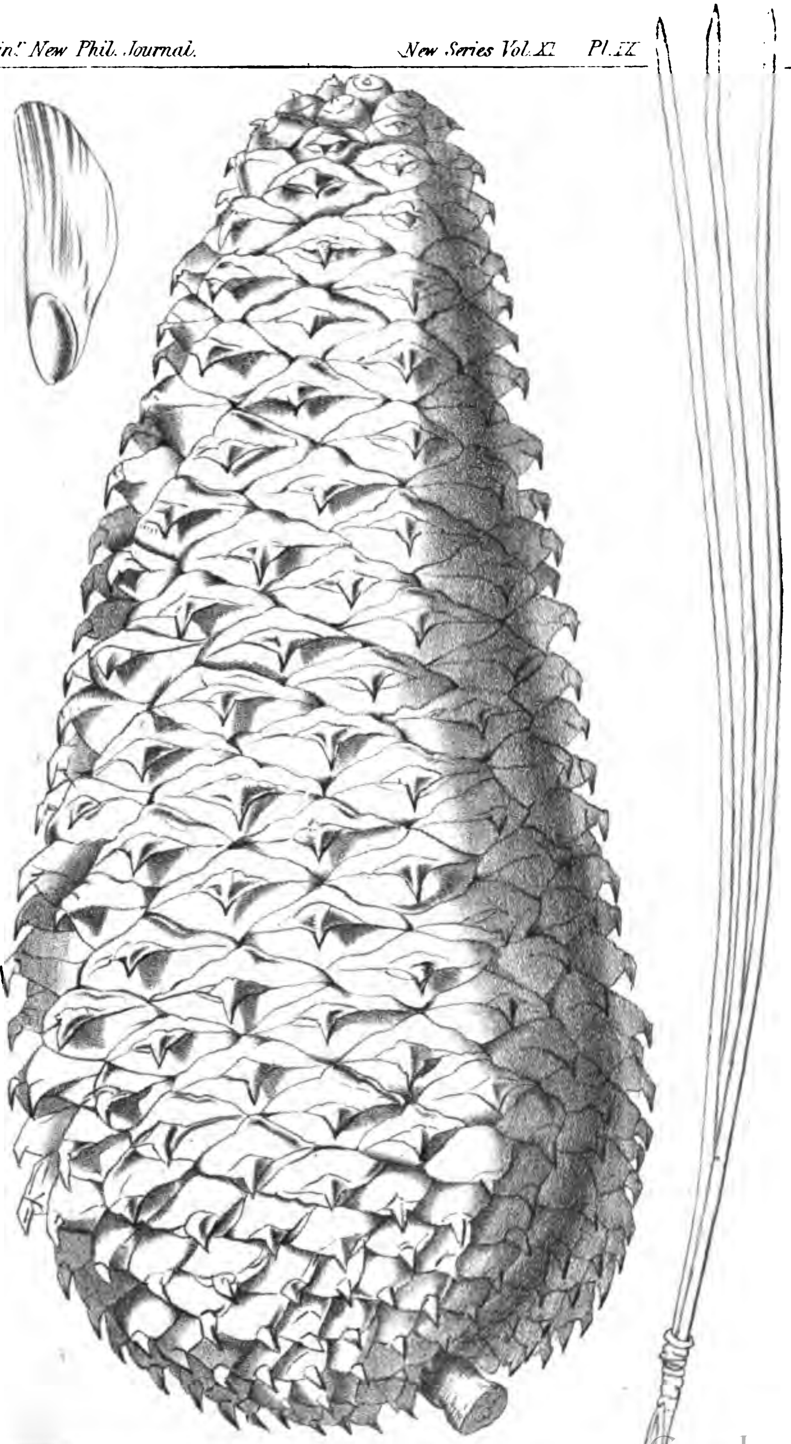

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